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Editorial Notes*

THE Stone of Scone is a rectangular slab of sandstone, probably derived from rocks near Scone (Perthshire) of Devonian age—which is about 300 million years. For the last infinitesimal portion of its existence the Stone has been used as a seat on which kings have been crowned. Last Christmas it was clandestinely removed from the Coronation Chair in Westminster Abbey, and at the time of writing it had not been recovered, in spite of strenuous and prolonged enquiries by the police. The purpose of its removal was political propaganda on behalf of the Scottish Nationalist movement. This act was greeted with shrieks of horror in the Press, though one writer to the *Times* thought it rather a lark. How should a civilized person react ?

The Stone itself, though artificially squared, has no interest apart from its historical associations. But these cannot be denied unless we are prepared also to disregard all objects and sites thus endowed, and this we are not prepared to do. Such associations have a sentimental value which it would be idle to deny ; they belong to the realms of fancy and emotion, not to those of aesthetic or intellectual appreciation, but then so do we ourselves in part. The most hard-bitten archaeologist or historian could hardly see the Pass of Thermopylae without a thrill, even though he might retain but the haziest memory of the tactics of Marathon. The Alfred Jewel derives much of its effective interest (and value) from its association with King Alfred, who ' had it wrought ' as the inscription tells us. Similar emotions may be stirred by the sight of a Roman road or wall, a linear earthwork or by the intact abandoned fields of the prehistoric Britons. Such sentimental associations are akin to the *mana* which primitive peoples regard as inherent in certain inanimate objects. Indeed our own feelings when confronted with such sites and objects are surely like those felt by the Australian aborigines about their ancestral sites, except that our ancestors were historical. *Mana* is a kind of objectified sentiment possessed in greater or less degree by certain things. Looked at thus the Stone of Scone is saturated with *mana*. Other things that the archaeologist has dealings with may attract him primarily on intellectual grounds, but few probably are quite free from *mana*.

* These Notes were written in March, before the Stone was recovered. They have been left unaltered.

We must then have some sympathy with the outraged feelings of those who have cried out about the removal of the Stone from Westminster Abbey. We consider, however, that the outcry reveals a somewhat primitive state of mind. For no such outcry has been aroused by many of the innumerable acts of vandalism perpetrated during recent years, and still being perpetrated. These acts have destroyed for ever monuments of British history and prehistory whose *mana*-content may have been relatively low but whose intellectual content was high. A truly civilized people would be able to appreciate the latter, and to set as high a value upon these other monuments *en masse* as upon one whose sole claim lay in what we have called its *mana*-content. As examples of such recent vandalism may be cited the obliteration by ploughing of the interior of Hod Hill camp (Dorset) and the desecration of its ramparts by barbed wire ; the bull-dozing of the Bronze Age barrows in Farway Hill (Devon) ; the levelling of two of the finest groups of prehistoric fields in Hampshire (Great Litchfield Down and Chilcomb Down) and of the hill-fort of Oliver's Battery near Alresford. If it is a question of sacrilege what are we to think of the threatened destruction by the ecclesiastical authorities themselves of the historic church of St. Peter's, Winchester ? These are merely a few examples within our own personal knowledge ; how many others are there ?

There is of course a distinction to be made between objects or sites which have association-interest only (such as battlefields) and those which embody human achievement and labour—between, for instance, the Stone of Scone and the Pyramids. Association-interest is a term applied to books that belonged to famous people ; such have thereby acquired an added value which is expressed in terms of money. But most people buy books to read, not to absorb the *mana* exuded by their associations ; only a collector would buy a Bradshaw because it belonged to Mr Gladstone. If, however, the book is one that the buyer really wants to read, then it seems there is a legitimately enhanced pleasure in possessing, say, the author's own copy or one with a famous name written on the title-page. But the book is bought primarily for its contents ; it is valued because it embodies human achievement in the realm of art or knowledge.

In order to make the comparison clear let us take an imaginary example. Let us suppose that there existed a copy of Bradshaw that had a very high association-interest, having belonged to a number of famous persons in succession, and that on the other hand the works of a dozen or more great writers existed in manuscript only. Suppose the Bradshaw were stolen and the manuscripts deliberately pulped ; which would represent the greater loss ? Yet the comparison between this imaginary example and what has just happened does not seem false or unduly strained. Bradshaw corresponds to the Stone of Scone, and the manuscripts to the sites and buildings which have been deliberately obliterated or destroyed. We have already given instances of the obliteration of prehistoric sites. Unfortunately there are others. In London alone there have been destroyed since 1918 the following buildings of architectural distinction :—Regent St. and the Quadrant (John Nash, 1820) ; Adelphi Terrace and 31 Soho Square (the Adam brothers, late 18th cent.) ; Waterloo Bridge (John Rennie, 1817) ; Clifford's Inn (14th cent.) ; Dorchester House (L. Vulliamy, 1851) ; Devonshire House (Wm. Kent, 18th cent.) ; Sir Joshua Reynolds' house in Leicester Square (late 17th cent.). There has also been

mutilation of such fine examples of architectural planning as Berkeley, St. James's, Queen and Torrington Squares. This mutilation still continues in spite of protests; Carlton House Terrace is now threatened by the bureaucracy.

The Scone affair seems to reveal a lack of any sense of proportion in the respective values of historic and architectural monuments. For we are at this very time witnessing the greatest and most relentless destruction of such that has ever taken place. All are irreplaceable and their loss impoverishes British culture. And it should be remembered after all that the Stone of Scone is probably quite safe and will eventually come to light again. But the architectural destruction and the insidious attacks on a series of lesser historic and prehistoric monuments, though intermittent, go on unceasingly; the latter, like coast-erosion, acts spasmodically and is hard to observe but infinitely destructive. We fear it must be said that a people has the historical monuments it deserves and can appreciate. If the will to preserve them were present and generally diffused amongst the population, they surely would not be allowed to perish. The fact that they do is damning evidence of a widespread philistinism that can destroy only and cannot replace.

Place-names: a review

by the Editor

1. The Place-names of the Isle of Wight. By Helge Kökeritz. Uppsala, 1940.
2. A Contribution to a Dictionary of Old English Place-names. By Rune Forsberg. Uppsala, 1950.
3. The Place-names of Cumberland, parts 1 and 2 only: English Place-name Society (Cambridge University Press), 1950. 18*s each*.
4. Horowhenua: its Maori place-names and their topographic and historical background. By G. Leslie Adkin.

SOME years ago the present writer had occasion, at a social gathering, to mention the subject of place-name study, one of the foremost exponents of which happened to be one of the party. His remark was met with a blank stare, followed by the question 'What *is* place-name study'? Such illiteracy has deep roots in the English cultural tradition which owes more to the dilettante than to the professor. The understanding of place-names demands some knowledge of the early languages spoken in this country (Celtic, and Anglo-Saxon—or, as it is now called, old English); and that is acquired in universities rather than in country houses and parsonages. But it also demands a familiarity with topography such as can best be acquired by a native who is permanently resident and knows the lie of the land at first hand. The best results are obtained from a combination of both, of book-learning enlivened by field-work.

The prime object of place-name study is to discover the origin and meaning of existing place-names, and, as a secondary objective, to draw inferences from them that will throw light on the early history and social organisation of the region studied. Except in the southwest, where Celtic names are most numerous, and in the north, where there is a strong mixture of Scandinavian forms, most English place-names originated with the Anglo-Saxon colonisers of the 5th and 6th centuries. It follows that one of the chief sources of knowledge is the fine series of land-boundaries attached to charters of the pre-Conquest period. These are mostly in Anglo-Saxon, but the vocabulary is limited and the common form easily mastered. The unit is usually either the existing parish or a unit of comparable size. Every book on English place-names makes full use of these land-boundaries, for in order to discover the meaning—'derivation'—of a word it is essential to know its earliest recorded form. For instance, the place called Slaughterford near Stow-on-the-Wold, Glos, has no connection with a battle, for it was spelt *slohtran ford* in A.D. 779 and means 'the muddy ford', from O.E. **slohtre*. When asked to explain the meaning of a name, the place-name student will always demand to know first the earliest recorded form; he *may* then be able to give a satisfactory explanation, but he cannot possibly give one without this knowledge.

So much for the indoor approach. But, though essential, it needs to be supplemented by field-work if errors of interpretation are to be avoided. That is of course fully recognised by place-name students, but it is an ideal which is not always attainable in practice. Nor would even the most ardent field worker suggest that it can ever be otherwise. The proper function of field-work in place-name study is to test meanings and identifications by actual inspection on the ground; it is subsidiary to the purely

philological approach and not (as in archaeology) primary. Having conceded so much, one may add that a place-name student who has never emerged from his study can never have the fullest comprehension of his subject.

A good example of this need for field-work is provided by the O.E. word *mere*. Dr Kökeritz allows that this may be used of ponds, though he does not give this word as an equivalent in his glossary (p. 273 : ' lake, mere '). Dr Ekwall in his Dictionary gives the same two words as a translation of *mere*, but fails to realize that, on the Wessex downs, where there are no lakes and never were, *mere* always means ' pond '. Some of his explanations, therefore, need revision. There could never have been ' lakes ' at Buttermere (Wilts), Dummer (Hants) or Ashmore (Dorset), but the ponds are there still. Most final and conclusive is the evidence of *meres* which are mentioned as O.E. boundmarks and are also still to be seen, such as Rushmoor Pond on the bounds of Bradley (Hants) and Rockmoor Pond where Hants, Berks and Wilts now meet.

Field-work again was responsible for identifying the 10th century *styrian pol* of some North Berkshire bounds with a still existing pool in a wood not far from Oxford[1]. It thereby amplified the meaning of the word *pol*, proving that its connotation covered artificial fish-ponds as well as ' deep places in a river ' and ' tidal streams ', the definition given by Ekwall. Field-work based on O.E. boundaries has given greater precision to the meaning of other water-words such as *seath*, *æwiell*, *floda*. It is found that, although both *seath* and *mere* are used of artificially constructed ponds, the terms were mutually exclusive ; *meres* are found to be ponds of still water, usually on upland plateaux and filled by rain, *seaths* to be on low ground or valley bottoms and filled by springs.

Another word whose meaning has benefited from the work of field archaeologists is O.E. *baec*, which Ekwall translates ' back, probably also, like OHG *bah*, ridge[2] '. There seems, in spite of Ekwall's statement, to be some reluctance in admitting the existence of this word ; yet it is not uncommon in the O.E. bounds, and in those I have examined the translation ' ridge ' is often preferable to that of ' stream '. It survived, as a dialect word ' batch ', down to modern times. John Aubrey (1626–97) testifies to this in his *Monumenta Britannica* where he says : ' At Zoyland in Somersetshire are the *Batches*, on which Collines now stand fower Parishes. Mr Andrew Paschal, B.D. In this country they will say, on the Batch of the hill, meaning the Ridge of the hill : query, if it is British for a Ridge '. The modern map of Somerset contains many instances, such as Castle Batch (a medieval castle mound), exemplifying this use of the word.

Yet another is the O.E. *hlinc*, which Ekwall defines as ' a bank separating strips of arable land on a slope, a rising ground, ridge '. It is common in field-names, usually in the form ' lynch ', or the diminutive ' lynchet '. It is very common in the O.E. bounds, and in those of Wessex it invariably has the first of Ekwall's meanings. When the bounds mention *hlincs* they are always skirting the arable fields of the community. Ekwall's definition needs amplifying by the statement that the bank is not one deliberately formed (with a ditch beside it) but the natural accumulation of soil that forms on the

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[1] ANTIQUITY IV, 1930, 480–3.

[2] It is, in my opinion, quite certain that it is this word *baec*, and not *baece*, *bece*, a stream, which enters into the name Burbage, Wilts (*andlang burh beces*, BCS.1067 ; *with baeces* [*burhbeces*, B.], BCS.1213. The boundary here follows the watershed between the Kennet-Thames and the Salisbury Avon, where it is geographically impossible for a stream to have existed : see my remarks in *Wilts. Arch. Mag.* XLI, 1921, 287.

I had collected many instances from the modern maps where ' batch ' seems to denote a hill or ridge, and hoped one day to publish them, but they were destroyed in the war.

edge of a cultivated slope. Dr Kökeritz omits the explanatory part of Ekwall's definition, but the word is rather uncommon, it seems, in the Isle of Wight, only occurring three times in his book, nor can I recall seeing any good examples of medieval cultivation-terraces in the island, though doubtless they do occur.

The books whose titles are given at the beginning of this article are all of them scholarly works of great merit. Dr Kökeritz knows the Isle of Wight at first hand, and he has added a unique and pleasant feature to his book by publishing photographs of some of the places whose names are dealt with. He makes out a strong case for rejecting my identification of the *Wyghtbergh* of the 14th century beacon-list with *Wihtgaraburh* and Carisbrooke Castle[3]. One of the chief objections is the absence of any (other) mention of Carisbrooke Castle by this name, while there are many others, from the 12th century onwards, all of them variants of the modern names Carisbrooke. (The last came from the stream, now called the Lukely Brook, and has no connection with Wihtgaraburh). The problem is difficult and far too complicated for discussion here. Dr Kökeritz admits that *Wihtgaraburh* may well be identified *topographically* with an earlier fort on the site of Carisbrooke Castle, and cites a Ministry of Works guide-book mentioning ' the remains of a late Roman fort, the walls of which can be seen '. He has overlooked my note on it[4], and the illustration there published which shows a pre-medieval stone wall with a round bastion, both quite certainly Roman work. There is therefore no need to postulate ' an old Celtic fort ', for even had such existed it would already have been superseded and probably obliterated by the Roman one when the Jutes arrived. He concludes that *Wyghtbergh* meant ' the white hill ' and referred to one of the high downs round Whitcombe which means ' the white valley ', or that it ' was the original name of Alvington Down, on the slopes of which there are chalk-pits and near which we find Whitepit and Whitelane Homestead ' (p. LVI). The term *bergh* should come from O.E. *beorh*[5], which almost always denotes an artificial burial-mound or barrow, and when it does not, as in Black Barrow, Kingston, denotes a small conical hill, not a flat-topped down. I know of no instance anywhere where *beorh* or the modern word barrow are applied to such flat eminences. But *Wyghtbergh* might well have denoted a barrow on such an eminence.

The name of the island itself is Celtic. The first mention of it is in the classical writers who call it Vectis. A 13th century manuscript of Nennius equates the British name ' Gueid vel Guith ' with the Latin word ' divorcium ', meaning (it seems) ' a fork in a road, a watershed '. Dr Kökeritz regards this as ' a sense that would suit the situation of Wight very well indeed '. Presumably it originated from the fact that voyagers going southwards down Southampton Water and coastal ships had to fork right or left to go round it. As names like Wight were probably given not by the inhabitants themselves but by the inhabitants of the mainland, the explanation seems reasonable.

One of the most valuable features of Dr Kökeritz's book is his full treatment of the O.E. bounds, to whose identification he has made some important fresh contributions. Here one may mention a confirmation from field-work of just 30 years ago. Grundy, working from the map alone, identified the *gemot beorh* (assembly or moot hill) of the bounds of Calbourne[6] with ' the tumulus which stands on Newbarn Down about 1¼ m.

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[3] See ANTIQUITY V, 1931, 457.

[4] ANTIQUITY I, 1927, 476.

[5] An original *burh* is not absolutely excluded ; the two words were sometimes confused in Middle English.

[6] BCS. I, 392, A.D. 826, *Arch. Journ.* LXXVIII, 136 ff.

S. of Ashen Grove'. This is in fact the southernmost of a row of three, of which two only are marked on the map (94 S.E.). A furlong due south of it, and also on a parish boundary the map marks a pit by hachures, but it is in reality a small rectangular enclosure within earthen banks, the sides being 23 by 15 yards in length. This is closely comparable in shape and size with the well-known moot at Cut-Thorn on Southampton Common, and may well have served the same purpose. It stands at the junction of three parishes (Brighstone, Shorwell and Carisbrooke) and not far from another parish boundary congruence involving Calbourne.

Dr Kökeritz's activities were bounded by the sea. Those of Dr Forsberg were confined within the arbitrary limits of the letter L. His work was intended to form part of a dictionary of Old English place-names contemplated by the late Professor Zachrisson; but the scheme fell through, and some of the work done for it has, fortunately, been published here in a different form. The result is a valuable collection of names and comments thereon, whose reading, though a pleasant and absorbing task, is not easy. Abbreviations are so freely used that, until one grows familiar with them, reading is difficult; but that is perhaps inevitable in what was originally compiled for a dictionary.

Dr Forsberg's 'terms of reference' were wide enough to enable him to deal (under *-lett*) with such words as *scirlett*, involving a full discussion of the meaning of *scir*. He quotes Ekwall's explanation of Shirley as '*leah* belonging to the shire' or '*leah* where the shire moot was held', but wavers between this interpretation and one involving O.E. *scir*, bright. Evidence which he could hardly know proves conclusively that Ekwall's second interpretation is correct; for Shirley near Southampton, Hants, was the *leah* on which the shire did actually meet, in the Cut-thorn enclosure mentioned above. Southampton Common originated the name of Shirley. *Scir* may of course denote smaller divisions than a county, but as the adjacent town of Hamton gave its name to Hampshire (Hamtunscir, A.D. 755) it is fairly safe to infer that the town was at the least an important county centre as early as the 8th century and a suitable place for holding shire moots. It is also, in fact, far more central geographically than might be thought for all Hampshire except the region beyond Basingstoke—then thinly inhabited and mostly forest.

The author has of course made full use of the O.E. land-bounds, and a valuable appendix of 17 pages is concerned with some doubtful or erroneous identifications. It is a pity that he did not include the Ashbury group in North Berkshire, correcting the confusion of Grundy's work thereon. (*Aescesburh* as the Rev^nd. Charles Overy has shown is Uffington Castle). Perhaps the fortuitous absence of words with initial L may be partly responsible. On pp. 204–5 he discusses two sets of bounds (Birch 775 and 781) of land at Winterbourne and Mapperton, Dorset, which reveal that the old name of the linear earthwork now called Combs Ditch was *cunucces* or *cunnuces dic*. This is a Celtic personal name, occurring as Conuc in the Book of Llandaff[7] and in several English place-names. Does the name go back to the period of construction? The dyke is thrown across the main road from Salisbury to Dorchester, an ancient thoroughfare, and might well have been made by the Celtic inhabitants against the threat of Cerdic's invasion. We know that Cerdic turned northwards after the battle of Charford, and the survival of Celtic culture in Dorset is proved both by the absence of pagan Saxon cemeteries and by the many Celtic place-names in that county.

[7] See my article in *Arch. Journ.* LXXVII, 1920, 145. Cunictune (Conington in Cambs., not Hants.) should be omitted.

Dr Forsberg has also published a separate article[8], identifying some hitherto unidentified, or wrongly identified, charters (Birch Nos. 158, 499, 663, 726, 923). It should be stated, for the benefit of those unfamiliar with the sources of place-name study, that the charter-identifications of both Kemble and Birch are quite untrustworthy. During the half century that has passed since Birch's *Cartularium* appeared much work has been done both here and abroad, and one would imagine that by now most of them have been correctly located. What is now needed more than anything else is a topographical index, arranged under counties, of at any rate all those charters to which boundaries are attached. This should cover all those in Birch and Kemble and also those others (not many) which are not found there. There should also be compiled and published in the same place a list of such as cannot be identified. That alone would be a step towards their identification, for it would bring them to the notice of students interested in a particular region. The present writer has played about with these O.E. boundaries in an amateur way for some forty years, and at an early stage he found it necessary to compile such an index for his own use. It was necessarily a most inadequate affair, and it was lost in the war, but it did prove extremely useful. Cannot someone compile a better one ?

We in this country are deeply indebted to the members of the great school of Swedish place-name students for all they have done for us. They know, from personal contacts, how strongly that is felt by the few English students directly concerned with the subject. But surely some wider expression of that feeling is due from the representatives of learning in this country. There are several obvious ways in which that appreciation could be expressed, and the neglect to take them must remain a source of embarrassment to the few.

The reappearance of the once annual volumes of the English Place-name Society will be warmly welcomed, not least by the faithful who have not wavered in their allegiance during a decade of drought. A review of these two Cumberland volumes is difficult for several reasons. They cover an area with which the reviewer is quite unfamiliar ; they do not contain the usual introduction (which is promised in the third and concluding volume) ; and they deal with names in three different languages (Celtic, Norse and Old English). The region is one that can only be tackled by more than one specialist, for it is beyond human powers to master both Celtic and a Teutonic language. We admit being disappointed at not having any reading matter in the first volumes received after so long a break, but that we hope will soon be put right. At the risk of harping on an old complaint, we must criticize the absence of any clue to the present address of the Society. We should like to help its work by recommending our readers to join it—and we do so most heartily—but how are they to set about it ? Even the publishers modestly conceal the exact whereabouts of ' Bentley House, N.W.1 ', a region containing many other houses. We ought, of course, to know instinctively where the General Editor lives, but we don't, and have forgotten. This geographical coyness seems unnatural in a book exclusively concerned with place-names.

It is a far cry from Cumberland to Horowhenua in New Zealand, whether one travels in space or time. From the place-name student's point of view the time distance is about a millennium and a half ; for it is not stretching cultural parallels too far to compare the present situation in New Zealand with that in Britain about the end of the 6th

[8] Namn och Bygd : tidskrift för nordisk ortsnamusforskning, Vol. 30 (1942), häfte 3-4, pp. 150–58.

century. Two languages then co-existed[9], and it would still have been possible for a Saxon monk to do for the Celtic place-names of Britain what Mr Leslie Adkin has done for the Maori names of a district in New Zealand. Even as late as the 9th century the Welshman Asser, friend and biographer of King Alfred, could rescue and record a few still surviving Celtic place-names in southern England. The paucity of that record is, by contrast, some measure of the value of Mr Leslie Adkin's book. For not only has he set down and documented a very large number of names, and given the explanations of them, but he has indicated their positions on the ground by a series of neatly drawn maps. He has also given quite a large amount of new and purely archaeological information.

We are so accustomed to thinking of New Zealand as a ' new country ' that we are apt to forget that it has a history, of a sort, reaching back nearly to the time of the Norman Conquest of England. The process of colonization by the Maoris was still going on during the 19th century, and, except that the fighting was exclusively with other colonists—for the islands were uninhabited before—the process may have been not unlike the Anglo-Saxon colonization of England. The origins of the place-names they gave is therefore of considerable interest to us here. It has been pointed out before in another ' new ' land (North America) how chance plays a great part in deciding place-names. That is of course allowed for by place-name students, even though in the nature of things they cannot in such cases explain the origin. But most of the Maori place-names, like those of almost any other country, are either personal or topographical. ' As in other districts, names indicating personal possession, or the abode, habitation, or tarrying-place of some person, usually a notable, are fairly common '. Some are said to be ' descriptive and poetic ', and others are traced to a particular remark at a particular time. We wonder whether folk-etymology might not better explain names of both classes ; there has been plenty of time for it.

An interesting section on native trade-routes would have been improved by a sketch-map showing them in relation to *pa's* and physical features. The ' war-trails ' seem to correspond exactly to our *herepaths*. Some of these tracks have left visible marks on the ground ; it would be well worth while following up these traces, and we commend this suggestion to New Zealand hikers in search of an objective. The detailed description of one such track—and ' detailed ' is the operative word—would be well worth publication.

Mr Leslie Adkin has earned the gratitude of all students by his collection and publication of a mass of facts that otherwise would have been lost for ever. In the years to come, when such work is no longer possible and when all traditional lore has been forgotten, historically minded New Zealanders will be even more grateful to him and to all those who have contributed to the compilation and publication of the book.

NOTE.—Mr Adkin has published also an article in the *Journal of the Polynesian Society*, Vol. 59 (March, 1950), pp. 1–34, entitled ' Supplementary Data relating to the ancient Waitaha in the Horowhenua-Te Whanga-nui-a-Tara area, North Island, N.Z.'

[9] It is often forgotten that two Celtic languages are still spoken in Britain, and that two others (Cornish and Manx) are only recently extinct. The present writer has encountered people in North Wales and in the Outer Hebrides who neither spoke nor understood English.

'Jews' Houses'

by Cecil Roth

IT is well known that there are numerous ancient sites and buildings in this country whose nomenclature brings (or brought) them into hypothetical association with the Jews. It will be best, therefore, to begin this enquiry by drawing up a provisional list:

The Jewry Wall, Leicester.

Market Jew, Cornwall.

Jews' Mount, Oxford. H. E. Salter, *Oxford City Properties*, 1926, 207–12.

The Jews' Hall, Winchelsea.

The Jew Gate, Newcastle: otherwise Silver Street, a name dating at least from the 17th century. *Archaeologia Aeliana*, 2 Ser., xxiv, 156–7.

The Jews' Meadow, Horsham. *N. & Q.*, 1936, i, 27.

The Jewry, Martley, Worcs.: an Elizabethan house.

To the above must be added sites no longer identifiable on the ground but known from historical records:

The Jews' Way, Bury St. Edmunds: Gibson's *Camden*, p. 451.

The Jews' Houses, Southampton: *Rot. Orig. Abbrev.*, ii, 103 b [1282]: J. S. Davies, *Hist. of So'ton*, 1883, 456. They were situated just outside the S. gate of the Castle.

The Jews' Tower, Winchester. *Liberate Rolls*, 1249, pp. 235–6.

Jews' Tin } Cornwall. Max Müller, *Chips from a German workshop*, iii, 320.
Jews' Houses }

With some ingenuity one could associate some of these names hypothetically with Jewish experience. Thus I formerly imagined that the Jews' Tower at Winchester might have been that portion of the Castle where Jews were allowed to take refuge in time of danger, or which they helped to defend on such occasions [1]. The same might have been true of the Jews' Mount at Oxford, although the name is only attested from a post-medieval source [2]. But in some of these cases such an explanation is impossible. The tiny settlement at Bury St. Edmunds came to an end in 1190, and it is out of the question that it can have left its imprint on local nomenclature outside its area of residence. There was never any Jewish community in medieval Southampton, though one or two Jewish residents may be traced here, and it is just possible that in the sea-ports there may have been some place of resort for Jews who embarked or disembarked. The same applies to Winchelsea, with the added reservation, however, that the ruins of the Jews' Hall are of 14th century architecture—belonging, that is, to the period after the expulsion of the Jews. Only a single Cornish Jew is on record throughout the entire Middle Ages, so far as my notes show. The outstanding case is Leicester where once again the medieval Jewish settlement was minimal, and the existence of a 'Jewry' in the accepted sense is out of the question; there is no doubt whatsoever that the Jewry Wall here—the most impressive relic of antiquity in the town—is in fact pre-medieval, belonging to the Roman Forum[3].

[1] See my *History of the Jews in England*, p. 103.

[2] I advance the hypothesis in my new book on the *Jews of Medieval Oxford* (Oxford Historical Society, 1951), 109–10.

[3] Compare the impressive work on the subject newly published: 'Excavations at the Jewry Wall site, Leicester', by Kathleen M. Kenyon. S. of A. Research Committee's 15th Report, 1948.

It is important to note that this series of English names is not isolated. Parallels may be found on the Continent on a far wider scale and with a more ambitious application. In France, for example, there are many place-names embodying the word 'Juif' or something like it: e.g. Villejuif, near Paris; Baigneux-les-Juifs, Côte d'Or; La Roche aux Juifs, Orleans; and many more besides. Similarly there are in Germany many place-names like Judenberg or Judendorf, etc., or like Zydaczów in Poland. Somewhat forced attempts have been made to explain these names; those in France as an indication of very ancient rural settlement[4], those in Germany as the memorials of farms in Jewish hands in very ancient times before the Jews were compelled to become urban dwellers; or even as indicating a chain of Jewish settlements extending in the Dark Ages from the Mediterranean to the Tyrol and into Austria [5]. Even further afield, in the island of Kythnos or Thermia in the Cyclades there is a group of ruins called by the villagers Hebraeokastron or Jews' Castle: an island in the Saronic Gulf with traces of fortifications is called Evraeonisi or Jews' Island: near Marathon is Ovriokastro (Ebraeokastro), or Jews' Town, the ancient Rhamnus [6].

It is common to all these places whose names are compounded with the word 'Jew' that there is no historical evidence that Jews lived there. This is above all true of what is perhaps the most remarkable instance of such association of names, which by reason of its artless narrative simplicity has not hitherto been taken into account in this connexion. The 12th cent. Arab geographer Edrisi speaks of Tarragona in Spain as 'a city of Jews'[7]. But so far as we know the Jewish settlement there was never of exceptional importance nor large enough in proportion to the total population at any given time to justify this description. Not long after Edrisi's day Benjamin of Tudela, the Jewish traveller and his younger contemporary, was at Tarragona, and would certainly not have failed to mention so remarkable a fact in Jewish life. But he does not even mention the Jews, merely stating: 'Thence [from Tortosa] I went two days' journey to the ancient city of Tarragona which was built by the giant Greeks; there is no similar construction in the whole of Spain'.[8] He is of course referring to the massive fortification walls which still survive and distinguish Tarragona [9]. It is, I think, obvious that Edrisi's phrase 'a city of the Jews' is the equivalent of Benjamin's 'built by the giant Greeks', both expressions in fact loosely corresponding to the term 'Cyclopaean' in vogue today—or yesterday—to describe massive structures of great or even mysterious antiquity.

Here it seems we have the key of the problem under consideration. It has already been suggested that in the Greek islands the terms Hebraeokastron was 'applied in contempt by Greek peasants to any ancient building whatsoever erected by strangers': this may now be amended to 'any ancient building of unknown origin', without any connotation of contempt. In England this formula explains the names assembled above quite satisfactorily; they must have been given for the most part to old structures of unknown origin which for that reason were associated in the popular mind with 'the Jews'. Above all, this rather than any hazardous folk-etymology provides at last a satisfactory explanation of the mysterious term 'Jewry Wall' in Leicester which answers perfectly to the definition suggested above.

The same formula explains the Continental names. There is no actual direct

[4] R. Anschel, *Les Juifs de France*, 41–57 ('Toponymie Juive').

[5] Cf. Baron, *Social and Religious History of the Jews*, II, 12.

[6] Murray's 'Handbook for travellers in Greece', 1854: W. Ettinghausen (Eytan) in the *Jewish Quarterly Review*, N.S. XXXVI, 1946, 419–21.

[7] *Geography*, II, 235. [8] *Itinerary*, ed. Adler, p. 2. I have revised the rendering.

[9] For an illustrated account of these, see ANTIQUITY, X, 1936, 72–6.

evidence of Jews owning villages in Western Europe in the Middle Ages (except perhaps temporarily, as the result of financial transactions), and still less of their constituting rural settlements—though no doubt there were some who were settled on the land until prejudice and legislation made this impossible. I suggest, therefore, that the various places with names like Villejuif, Judendorf, etc., were ancient sites probably containing obvious relics of great antiquity which were associated in legends with the Jews. It was to this that they owed their names, though later on some of them (e.g. Judenburg in Styria) postulated a more intimate association by embodying a Jew's Head in their coat-of-arms.[10] Of course this explanation does not cover all cases, and it may well be that the Jews' Meadow in Horsham, for example, was once in fact owned by a Jew, and that Zydaczów was an actual settlement of Jewish pioneers in Poland. My hypothesis, however, provides a valid explanation for the numerous cases where such an explanation is quite out of the question.

A literary parallel to this usage may be found in a well-known passage of Chaucer

> And over that a fyn hauberk
> was all y-wroght of Jewes werk[11].

There is little evidence of Jewish armourers in Western Europe in the Middle Ages—certainly not of specially skilled armourers—and still less of Jewish connoisseurs in armour. It is certain, I think, that this indicates work of special skill and quality, such as was to be associated with those legendary artificers. The same rather than any special technical allusion to Jewish methods of manufacture, may perhaps be the interpretation of the 'Jewish glass' (vitrum plumbeum, judaeum scilicet) and 'Jewish tapestry' (judaica vela) of which we read in the post-classical period[12].

In a way, my hypothesis does not so much solve the problem as push it a stage further back. The Jewry Wall and such like may not have been constructed by Jews; but the name suggests that there was a period, far back in antiquity, when the Jews in Europe were in fact renowned as builders, so that later all kinds of structures of unknown origin were attributed to them. In the 14th century Jews were not the outstanding armourers; yet Chaucer's phrase suggests that the tradition of Jewish proficiency in such handicrafts was still alive in the popular mind. The nomenclature thus seems to afford a fleeting glimpse of an era in European history—presumably in the Dark Ages—when in fact the Jews had an unquestioned pre-eminence in such arts[13].

The considerations put forward above do not, of course, affect the fact that there are authentic medieval sites and houses associated in popular memory with their former Jewish owners. Outstanding among them in this country is certainly the Jew's House at Lincoln, owned in the 13th century by Belaset, the Jewess of Wallingford, which stands next to the Jews' Court, almost certainly the medieval synagogue[14]. But here, too, a reservation must be made. The 'house of Aaron the Jew' opposite this in Steep Hill, Lincoln, though indubitably of 12th century origin, has been associated with the Jews and with specific Jewish ownership by a 'legend', the origins of which (as has now been demonstrated) are certainly not older than the 19th century[15]!

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[10] It is conceivable that the vaguely Jewish nomenclature of other places (e.g. Moyses' Hall, Bury St. Edmunds; Synagogue Well, Frodsham, Cheshire) may be due to the same association of ideas.

[11] *Canterbury Tales*: Sir Thopas, 2053–4. [12] F. Landsberger, *History of Jewish Art*, 166, 192.

[13] Might it not also be explained as 'omne ignotum pro Judaico'? Ed.

[14] As I showed in 1934; see my brochure, 'Medieval Lincoln Jewry and its Synagogue', published by the Jewish Historical Society of England. Belaset of Wallingford, who was executed in 1279, is not, as I then thought, identical with Belaset, daughter of Master Benedict of Lincoln, one of the most eminent of medieval Anglo-Jewish scholars.

[15] J. W. F. Hill, *Medieval Lincoln*, Cambridge, 1948, 217–23.

Homer and the Monuments: a review*

by Sir John Myres

THIS really monumental book reviews the material evidence accumulated—in more than two generations—for the periods within which the Homeric Poems came into being, and applies this evidence to discover the circumstances in which they achieved their actual shape. Outlines of this enquiry were proclaimed by Schliemann's excavations at Hissarlik (his site for Troy), Mycenae, and Tiryns; a remoter background by Sir Arthur Evans' work at Knossos; but detail has come from many hands, and in many fields of research—literary and even linguistic study reacting to archaeological, and propounding fresh problems to excavators. Two world wars have interrupted field-work and retarded publication. Miss Lorimer's own preface bears two dates (1946 and 1949), and she has to admit difficulties in obtaining important books, while some recent work is not noticed. In more than sixty years the leading figures in Homeric studies have changed; but one feels the abiding influence of Helbig and Dörpfeld in archaeology, and of Monro and Leaf in scholarship.

The opening survey of discoveries and archaeological results is admirable, and practically complete to 1946. While the precise relations between the 'Minoan' culture of Crete and the Islands, and the 'Helladic' of Mycenaean Greece are left discreetly vague, stress is laid on the new impulse represented by the 'grey ware' of Orchomenos, and its enigmatic contacts with Hissarlik; and even earlier links are noted between Euboea and pre-Hittite Alaça Hüyük. The Dimini culture of Thessaly is accepted as an offshoot from that of the Ukraine: this may need revision. Another northerly factor, recently established, might have been noted, connecting the Western Balkan lands with the neolithic Danubian region. All these contribute to a wider perspective for languages and even for literatures. If Odysseus began as a bear, as Rhys Carpenter asks us to believe, it is well to know how palaearctic that makes him. Miss Lorimer's cautious phrases about Anatolian folk- and cult-influences leave room for recent qualifications of Kretschmer's long-accepted conclusions about linguistic movements.

The long continuity detected in Attica, and the early contacts of Mycenaean Greece with Anatolia, find their significance in due course. The latter might be more confidently connected with the eastern aggressions of the Akhayawa in Hittite archives. It is a bold suggestion, however, that the shaft-grave folk at Mycenae may have been Hittite. Did they get their gold from Sardis? The puzzling distribution of cremation is examined farther in Ch. 3.

The *Foreign Relations* of this early world are treated separately. Except between Cyprus and Crete—perhaps over-fully presumed—little trace is found of oriental contact from the collapse of the Sea-raiders after 1200 B.C. to the Sargonid age, when the name 'Kimmerian' appears in Assyrian annals, and the Scythian 'reflex' bow and three-barbed arrow in the Aegean and the Poems. It is not quite clear why 'Alybe' in the *Trojan Catalogue* is thought to be 'late': there were silver vessels in 'Priam's Treasure'

*H. L. Lorimer, *Homer and the Monuments*. London, MacMillan, 1950, xxiii, 552 pages, 32 plates and 61 text-figures. £3.

at Hissarlik II and at Mycenae, and some have thought that Hesiod's ' Silver Age ' has its foundation in ' Minyan ' tombs. The ' Aeginetan Treasure ' is assigned to the 8th century ; but though its ' Phoenician '-looking objects have barbaric counterparts in North Italy, the Mycenaean gold bowl and other spirally-ornamented objects should be earlier. Or was this a mixed hoard, or loot from a cemetery ? The associated pottery is Mycenaean.

The upshot is that if Phoenician contacts are late, Phoenician references in Homer cannot be earlier than about 725–700 B.C., and ' Syrie ' in the Odyssey may be on the Syrian coast ; the new site Sabuni is suggested, situated at the mouth of the Orontes. This would fit the topography of Menelaus' journey—with the *Eremboi* as Aramaeans ?—and the visit of Odysseus to Cyprus. The Libyan journey projected by Odysseus should be after the foundation of Carthage, unless it is a ' traditional ' relic of Argonaut and Akhayawa seafaring. Since it is noted that *Cyrene* is an old Aegean word, we must wait, with Evans, for excavation. Nowhere in the Mediterranean have physical changes obliterated so much. In the West, it is suggested that Phoenician enterprises go back into the quiet interval between 782 and 745 B.C. ; but the ' Phoenician seapower ' is later (709-664 B.C.).

About *Cremation* and *Iron*, two cardinal innovations, much has been written, and Miss Lorimer is concise. Both are obscured by the queer distribution of the occurrences in the Homeric world and beyond it. But confusion of theories has subsided, and Ridgeway's name does not occur in these pages. Careful analysis, revised from *JHS.*, LIII (1933), shows that cremation had a very limited vogue, from the last end of Mycenaean culture—when it became the sole rite in Attica—to the end of ' Early Geometric ', when it almost disappears. Except one queer example at Prosymna, it is absent from Peloponnese, and at Syracuse it is rare in the early tombs. In Crete, however, it became habitual, and occurs at Cos and at Assarlik close by. At Colophon and Miletus it must have been introduced from Attica. The cremation tombs at Hissarlik VI and at Gurob in Egypt are earlier than all this, and distinct, unless the Achaeans learned the rite in the Troad, and the Akhayawa spread it as a war-custom. Attica too had a period of shifting population, and conditions are abnormal in the Epic. Yet ' the background of inhumation is there ' (p. 110). Miss Lorimer's cautious inferences are rightly set in the forefront of her story.

For *Iron* she gives a complete list of eleven Bronze Age occurrences in the Aegean, all within the period when the metal was in use in the Near East. One distant example from Vardarovtza may be of local origin but Aegean manufacture. The distribution is limited, with maximum frequency in Crete, ascribed to trade with Cyprus. At Halos iron seems to come in with cremation. In Homer, the conservative use of *chalkos* for weapons does not preclude the use of iron in similes, even on the battlefield. Only the famous iron-proverb in Od. 19 connects iron with violence. To make the ' self-cast ' ingot (Il. 23, 826) a meteorite is surely a counsel of despair. Iron, then, gives no hard-and-fast limit of culture but supports the evidence of cremation. Its discussion is rightly separated from that of weapon-types.

In the Aegean, *Writing* shows a long blank interval between the latest Minoan tablets about 1200 B.C. and the earliest alphabetic inscriptions. The ' folded tablet ' in Il. 6, 169–70 was ' scratched ', though the characters were ' signs ' not ' letters '. In what sense is this a ' carefully guarded allusion ' to script (p. 129) ? For the coming of writing into the Aegean, the converse appearance of Attic geometrical vases in Cyprus and at El Mina on the Orontes is significant : and as the earliest inscribed vase in Attica is about 725 B.C., the traditional record of an Olympic victor in 776 B.C. is not incredible. But

the Cypriote syllabary, derived from the Minoan ' A ' signs before 1400 B.C., was surely an obstacle rather than an intermediary. The great invention of vowel signs included the Pamphylian, Lycian and Carian scripts ; and, but for the word ' signs ', the ' folded tablet ' of Bellerophon suits this context, for it was to be read in Lycia. All these scripts, like Phoenician, took shape in knife-work on wood. Both for region and for date, the ' Rhodian Seapower ' of Diodorus should be brought into the picture.

Miss Lorimer rightly stresses Herodotus' statement about Ionian ' skins ' earlier than papyrus. *Byblus* only occurs once in Homer, for a rope (Od. 21, 391), whereas on Odysseus' raft (Od. 5, 258) the sails were Calypso's ' cloaks ', probably woollen ; the material of the rigging is not described. Later than 700 B.C. it would have been difficult to maintain the illiteracy of the heroes. Bellerophon's tablet is a first breach in it, and perhaps a ' novelty '. Stesichorus ascribed the alphabet to Palamedes, i.e., its introduction into Greece ; and Dosiades to the Cretans.

Within the limits of the Heroic Age and the Homeric Age, defined as above, progress can now be made with their material culture. Here *Arms and Armour*, *Dress*, and *Houses* are crucial. Each has had its own phases of controversy. Reichel's detection of Homeric references to the Minoan body-shield, in 1894, first troubled the waters. Though he ' grossly exaggerated ' the Mycenaean element in the poems, Miss Lorimer accepts Periphetes (Il. 15, 445–7) and Ajax of Salamis in several passages ; and Sarpedon's shield is ' clearly akin ' (Il. 12, 295). Surely some other lines are ' traditional ' phrases misapplied.

Reichel knew no alternative except the saucer-shaped hoplite shield with central arm-loop and a hand-grip within the rim : so he claimed for his body-shield all passages which did not indisputably refer to the other. It is Miss Lorimer's greatest achievement to demonstrate the long vogue of a third type—or series of types—of shield, from the Fall of Knossos to the rise of historical Sparta. It was a large circular convex shield, with a central boss and sometimes a spike, and a single cross-bar as handle. It was slung on a shoulder-strap like the body-shield, manipulated in the left hand, and supplemented by body-armour and leg-armour. Its antecedents are oriental, but it penetrated into Balkan lands and into Italy. Within the Aegean it developed the ' figure of eight ' shield of Boeotia, perhaps simulating the old body-shield. To these parrying shields, large or small, nearly all the disputed passages are applicable ; they make sense also of much of the chariot fighting, which originated likewise in the East, and persisted there and in Cyprus. Fuller treatment of the hoplite shield would have been welcome, but Miss Lorimer thinks it definitely post-Homeric, and has moreover already discussed its introduction, with the Homeric phalanx, in *BSA.*, XLII.

With the shield problem solved—for Reichel's theory ' contains a germ of truth ', and the hoplite shield is the ' best archaeological criterion ' for the completion of the Iliad—the *thorex* and other pieces of body-armour find their proper setting and equivalents, the reward of an immense and meticulous array of examples. Similarly for the head-gear ; Reichel's recognition of the boar-tusk helmet of Meriones is confirmed by fresh examples, while the unique sheet-bronze helmet from Dendra (Plates XIII, XIV) meets the ' all-bronze ' epithet in the *Odyssey*, to show how the long interval between Mycenaean and Corinthian headgear is to be filled.

Reichel died before publishing his examination of offensive weapons. Confusion caused by Ridgeway's misdating of Hallstatt and Villanova was cleared up by Peake, amplifying Naue's analysis of sword-types, and by new material from Knossos, Cyprus and Ugarit. Recognizing that ' epic tradition was perpetually renewing itself ' and that most epic words for sword-strokes are not precise, Miss Lorimer has distinguished, here

too, ' a few fragments of bronze-age poetry ', the ' silver-studded hilt ' and the ' all-bronze ' sword of Od. 8, 403–5, ' something of a curiosity ' and intended to be so, in far off Scheria. This recognition of ' novelties ' as deliberate enhancement of the old stories is a valuable line of criticism ; comparable with the naturalistic similes and the *genre* scenes on the *Shield of Achilles* and the *Brooch of Odysseus*.

Of the *Bow*, both types—European and Asiatic—are detected in the poems ; the former for popular use, the latter for heroes. The goats-horn bow of Pandarus, Miss Lorimer finds, has a ' specious appearance of precision which does not survive examination ', but she has transferred to the completed bow the length of the unworked horns. Pandarus, by the way, was not a Lycian but son of Lycaon of Zeleia. His bow was given him by Apollo (Il. 2, 827), but in Il. 4, 105 ff. he had shot the goat himself. Composite but not ' reflex ', it is ' curved ' in Il. 5, 9, and is 4, 124 it is bent ' round ' though drawn but to the breast (123). Only the Paeonians (Il. 10, 428 : 16, 848) have ' hooked ' bows ; only Odysseus' bow had a *gorytos* (Od. 21, 54), the Scythian bow-case : in Il. 10, 261 his bow had a *pharetre* (quiver) and could be used as a whip (10, 500), though it was *kampyla* (10, 353). This word, very widely used, can hardly mean more than ' flexible '. Apart from its *gorytos*, even the bow of Odysseus is handled and used like a western ' composite '. In particular he could inspect its ' horn ', which is concealed in the Scythian bow. The reflex bow, indeed, appears rather as a ' novelty ', like its ' three-barbed ' arrow ; and this was just possible about 700 B.C.

With the weapons come other metallic objects, the *Cup of Nestor* and the *Axes of Odysseus*, the latter referred here—as by Helbig and Monro—to the Amazonian type, another novelty, and rather vague. Miss Lorimer does not mention the perforated axe-blade of Vaphio and its cognates.

The *Chariot* was in continuous use in the Aegean from the shaft-graves to classical times, though latterly only in ritual. The diversity of construction does not affect its essential tactics. The Aegean four-spoke wheel is quite distinct both from the Egyptian six-spoke and from Hera's eight-spoke, which is an Assyrian ' novelty '. But this occurs on a Dipylon vase and at Clazomenae ; the six-spoke wheel too appears in geometrical Crete, and at Olympia. Note that Hector alone has *four* horses, in an interpolated passage (Il. 8, 185). Miss Lorimer's full discussion of early horses is really superfluous. There are horse-bones in Hissarlik I and II ; and horse-bits before 1500 B.C. It is rather at the latter end that chariots are significant, being antithetical to phalanx-fighting, and superseded by it. There are horse-bits again in early Miletus and other Asiatic settlements ; but surely they may be Lydian.

For *Homeric Dress*, too, the evidence is scanty and puzzling. Since garments decay, much of the argument is from fibulae and their position on unburned bodies. Here too between the coat-and-skirt of Minoan women, the complexity of which has been exaggerated (p. 369), and the rival Dorian and Ionian costumes of historic Greece, there is a long middle period, and in Cyprus, at least, Minoan and Ionian dress co-existed. Once more the lacuna is in Anatolia, and the zone of contact between Aegean and Asiatic seems to be on the sea-front, in the Greek settlements. Miss Lorimer thinks that women's dress in Homer is contemporaneous, which should mean that there were neither women nor their clothes in the ' tradition '. Homer had in fact few words in which to describe them. But more might be made of the supplementary introduction of linen underwear, always a crucial event among wool-wearers. Homer's *heanos* was what the Italians call *mutande*. For men's dress, the matter is simplified by earlier recognition of the tubular shirt, with its foreign name *chiton* ; and this too, in Scheria, ' went to the wash ' with the bed-clothes (*pharea*).

More too might have been said of the distribution of the earlier fibulae, which coincides with that of iron and of cremation, and goes back to the morrow of the Sea-raids, as the Cypriote evidence shows.

Miss Lorimer has, however, really made sense of the *Brooch of Odysseus*. As its place in the story required, it was a double ' novelty ' ; its construction is a rare and temporary fashion for clasps (not fibulae) in Etruria ; the quite inconsistent *genre* group of struggling animals is best compared with the contemporary fibula-plates of Sparta.

There might have been a note on Aphrodite's ' stitched strap ', a baldrick with pendant-charms, which is Cypriote fashion, and explains the hoards of amulets in many tombs.

A *tertium quid* in dress is propounded in the discussion of buttons, with Balkan examples ; here too it might be added that in modern Greece and elsewhere beads are freely used as toggles, like the double-cone ' whorls ' of the early Iron Age. Minor points are the denial of ' tasselled girdles ' to the Minoans (p. 384) ; the triangular fibula is not ' unmistakably Cypriote ' but Syrian ; and if ' wrapping round ' can ever be equated with ' diving into ' a garment (p. 378), vocabulary loses its meaning.

The *Homeric House* was fairly well reconstructed by Bassett and others a generation ago. But the superposed thresholds are not quite understood here. The ' ashen threshold ' is neither primitive nor clumsy ; it is normal in Athenian hotels, being the fourth side of a framed doorway. In a timber floor it can be sunk, though never completely ; but if the door-opening is of stone it cannot but project upwards ? The twin threshold of the Argive terracotta house is quite different. In discussing the *Removal of the Axes* (p. 511) Miss Lorimer seems to have overlooked Od. 24, 165, which gives what the Suitors thought.

Temples in Homer are like snakes in Iceland, if Athena's shrine in Ilium, with its seated image, be condemned as Athenian interpolation. But note that even at Athens the goddess received homage *standing*. Is this seated goddess another ' novelty ', and if so where was this curious shrine ? Perhaps actually at Ilium ? Here again, the poems stand between two worlds. Athena, Apollo, and Aphrodite have local ' dwellings ', and in Scheria there are temples and estates, as well as walls and houses, as in an 8th century Greek colony (p. 449).

Miss Lorimer's *Conclusions* from these separate and wide-ranging enquiries are stated modestly and in outline, but they carry us far. She recognizes a few vivid descriptions of Bronze Age objects, transmitted *somehow* from the 13th century or earlier. ' Epic tradition ' must have included these, and probably much else which has been modified or obliterated. At the other limit of date are the major interpolations, all just earlier than the adoption of hoplite armour, but later than the revival of intercourse with the East. The *Shield of Achilles* is a ' traditional set-piece ', originating in Mycenaean art under Cretan influence, but supplemented by ' scenes of actual life ' such as the ' City of Peace '. The wheeled tripods of Hephaestus, in the same context, are of the Geometric age, and earlier than the 7th century. The Olympians, whatever their origin, are handled ' in a completely Ionian spirit ', ' exempt from heroic restraint ', and liable to the criticisms of Leaf and Wilamowitz, but nevertheless they are the work of a great poet steeped in a traditional theology. Once upon a time ' Olympus ' was on Olympus. The *Funerary Games* are another traditional theme, remodelled to exhibit Achilles recovering his self-control.

The *Iliad* is thus ' in the true sense an epic ' (p. 490), resting however on earlier epics preserved through some organization at least as far back as the colonization of Ionia (p. 493).

On the topography of the *Iliad*, which has so exercised French scholars, Miss Lorimer has nothing to say ; but she has an ingenious compromise for that of the *Odyssey*. She distinguishes the larger landscape of the ' tradition ', with the home of Odysseus in Leucas, from the local detail of Thiaki-island to which Odysseus returns ; the poet having inherited an outlay earlier than the Corinthian colonization, and elaborated it from personal knowledge after that crisis ; and he reconstructed his Peloponnese from observations on a coast voyage. This is probably as good a solution as is possible. But the *Catalogue* in Il. 2, 631–5 does not suit either ' Ithaca ', and the voyage in Od. 14, 344 ff. suits both. Note that she finds Ithaca-Leucas again in the *Hymn to Apollo*, 427–9, in the 6th century. The island Asteris she finds not in Arkoudi but is Daskalio. Could it not however be in both ? She states a good case for 8th century composition by a poet who knew the *Iliad* and deliberately added to the *Tale of Troy*.

Both poems are thus presented as falling within one lifetime. At most the major additions are of the years following ' Homer's ' death, when his pupils inherited, edited, and wrote down what he had left. But ' epic tradition ', as the great poet received it in the age of Western colonization came down from before the Ionian migration, in some sort of guild, with a Mycenaean legacy. This is perhaps as near to historical truth as it is possible to go. Others have filled in details, and will continue to do so. But Miss Lorimer has not only contributed by masterly handling—not of the monuments only—to a real advance, but has very greatly facilitated the task of her successors.

A Technique for Surface Collecting

by Leslie Alcock

IT has usually been assumed that the collecting of surface material from the rich sites of Western Asia brought a harvest of knowledge at the expense of little archaeological skill; that while excavation, potentially destructive of evidence, required strict control, surface collecting was so harmless that it might be pursued indiscriminately. Recent survey work in Baluchistan has convinced the writer of the dangers of this view. This note is offered as a preliminary consideration of the problems involved in surface collecting, and the writer will be grateful for further suggestions from others more experienced than himself.

There are basically two methods of surface collecting. The first, which we may call the Stein method, consists of taking only those sherds with obvious decorative motifs, whether painted, stamped or incised. The method, limited in itself, has suffered in this country from a collectors' bias in favour of presumed chalcolithic sherds. The chief failing of the method is, of course, that by ignoring the plain wares which usually form the greater part of the equipment of a culture, it denies us important evidence about the broad nature of that equipment, and further deductions as to eating habits and every-day skills—for the same people may make superb painted pottery for religious purposes, yet be content with very inferior pots for ordinary use. Secondly, concentration on painted sherds may prevent us detecting industrial evolution and even cultural replacement, for traditions of painting may survive a radical change in plain wares. It will be shown on another occasion that the Harappan style of decoration lasts unchanging through major developments in the plain wares. And in Baluchistan we have in Sadaat ware decorative motifs which appear to form part of the Quetta ware tradition, and which were probably taken as such by earlier collectors; the associated plain wares, on the other hand, are quite different and excavation shows that Sadaat ware has as much claim as Quetta ware to separate cultural status.

The second, or American technique of surface collecting makes use of random samples—so random that they are best collected by untrained and uncomprehending labourers. The method has been used with some success in Peru to date single-period sites, when stratigraphical material and techniques were inadequate (kitchen middens and 25 cm. levels). But it is clear that as a method of dating multiperiod *dambs* where the material on the surface bears no proportionate relation to that below ground, it is quite futile. True, it gives a comprehensive picture of fabrics that is useful for scientific and statistical analyses; but these are frequently of no chronological significance. It gives us little insight into important human questions of function—were these thick gritty sherds part of storage jars or of bowls? is this fine ware used for goblets or beakers, and of what shape?

Life is too short, funds too limited, to permit most of us to indulge in the mechanical methods of smoothed graph curves based on mass analysis of tens of thousands of sherds, and we may be forgiven if we plead that intelligence too may be a useful archaeological tool. What is required is not a random sample, but one selected on rational principles, so that each sherd may be significant. Decorated sherds we must have: but we must balance them with plain wares, endeavouring to preserve the proportion between them

as they appear on the surface. And among the undecorated sherds, we must concentrate on those which reveal not only the fabric but the shape of the pot—rims, shoulders, feet and bases. Should there be any fabrics on the surface for which we can find no significant forms, then these too may be included. Finally, if (as on many Islamic sites) painted pottery is very rare it is worth making a very full collection of it, for the sake of its motifs, at the same time noting that the proportion in the bag is not the same as that on the ground. In this way we can, with the utmost economy, build up an accurate picture of the surface material of a site.

But is it enough to collect indiscriminately from the entire site, and lump all our finds together in one bag ? Different parts of a large and complex *damb* may have been occupied at different times, and there may even be a contouring of cultures, an archaeological treeline above which sherds of the early occupations are not found. This evidence could, and since it may be significant in planning an excavation it most certainly *should* be obtained when making a surface collection, the more so since sherds are not autogenous, and there is a very real danger of completely denuding popular sites. When the recent American excavation at Kile Gul Mohammed failed to produce either Zhob or Quetta ware sherds a very minute examination of the distribution of painted sherds on the surface revealed that the Zhob and Quetta peoples had settled to the south of the main *damb*. This fact had been concealed to a first glance by the thoroughness with which the assiduous collectors of twopenny coloureds had stripped the site of its evidence. Even so, had *we* possessed a ' drill ' for zonal collecting, we should have detected at the outset the chronological significance of the south-western terraces.

The chief difficulty is to decide on a suitable basis for one's zones. This is fairly easy in the citadel type of site common in Sassanian and later times. From the top of the citadel and from the sherd scatters which widely surround it, we may take evidence of the final period of the site ; at the foot of the slope we may look for evidence that the citadel utilized a pre-existing *damb* ; and on the slopes themselves we may find sherds of the intermediate periods. But this evidence must be used with discretion for on the summit of the Sassanian acropolis of Sra-kala, Pishin, we found sherds of Sadaat Ware, probably attributable to the early second millennium. Again, when a *damb* is split up into a number of loosely joined mounds, or into one main mound with terraces, we may collect from each separate area. It is the compact, circular *damb* which poses the real problem. Perhaps the answer is arbitrarily to divide its area into four or more segments and its height into two or more contour rings. In doing this it is useful to remember that later settlements tend to cluster on the sunny side or in the lee slope of earlier ones, and we should take account of such physical determinants in orienting our segments.

Finally it will be necessary to prepare a plan of the site on which the distribution of various sherd types can be plotted to examine its significance. Should time be very short, this can be done by pacing and by eye. But experience suggests that with no more equipment than tape, pocket clinometer, compass, and six or so ranging rods, an accurate contoured plan of a moderate sized *damb* can be produced in three hours. Surface collecting can proceed simultaneously, making use of the sectors and contour lines of the survey. Only by methods such as these can we bring into surface collecting the same critical standards as are already demanded of excavation.

PLATE I

XOCHICALCO, MEXICO (9th–10th cent. A.D.): see FIG. 1, p. 70

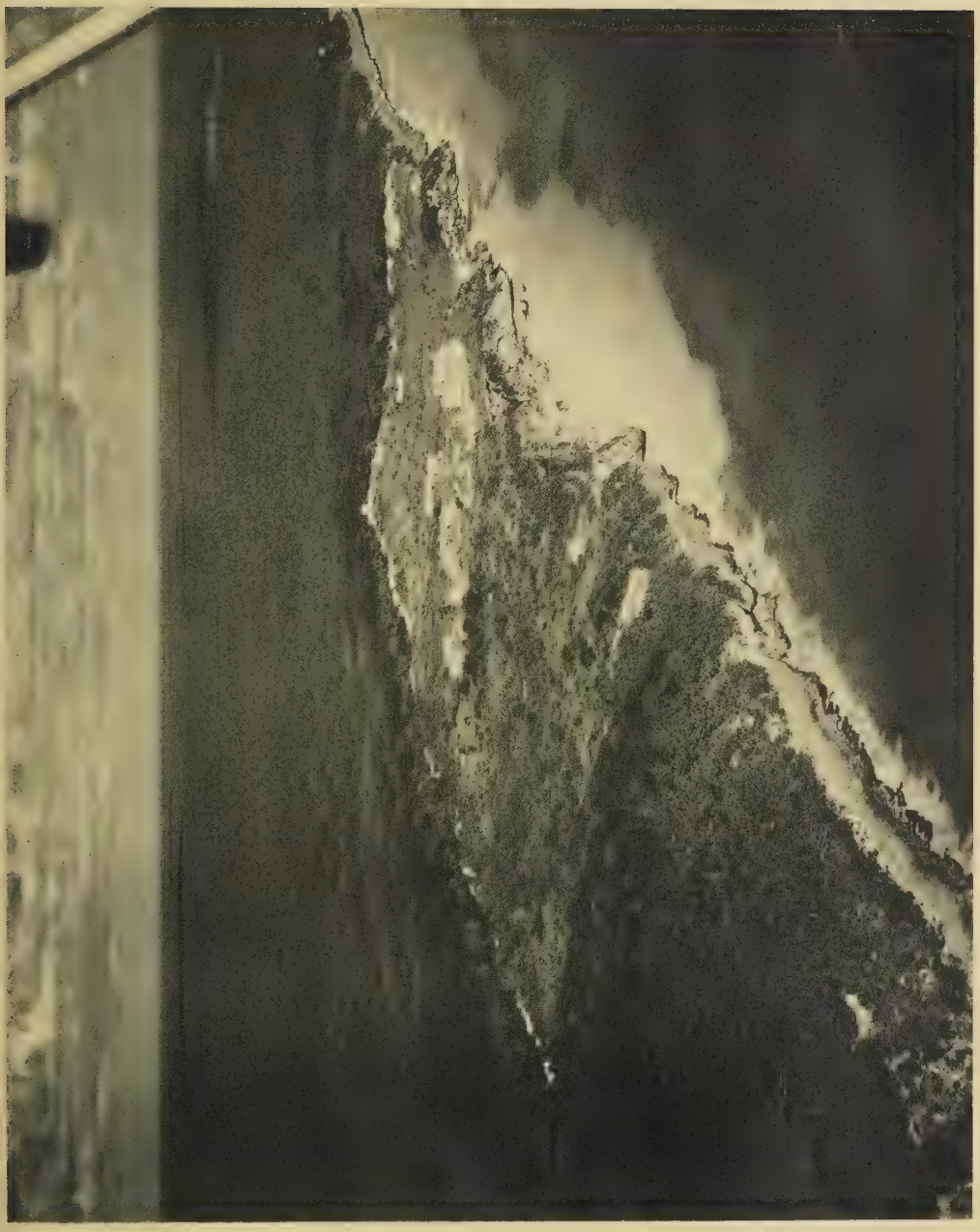
By courtesy of Compañía Mexicana Aerofoto

Scale about 1 : 10,000. North at top

facing p. 76

DEFENDED SITE OF CHUTIX TIOX, GUATEMALA
By courtesy of Carnegie Institution of Washington

PLATE III



THE WALLED SITE OF TULUM ON THE SHORE OF THE CARIBBEAN SEA, YUCATÁN

By courtesy of University Museum, Philadelphia

THE PRE-CONQUEST (i.e. pre-Spanish) ACROPOLIS OF LA QUEMADA IN WESTERN MEXICO
By courtesy of Compañia Mexicana Aerofoto

Mesoamerican Fortifications

by Pedro Armillas

THIS paper concerns the regions of Central and Southern Mexico, Guatemala, British Honduras, and the western parts of Honduras and El Salvador, which are recognized by archaeologists to have constituted a cultural unity in pre-Spanish times. This area was one of the two centres of civilization in pre-Columbian America, Peru, being the other. It includes the several cultural centres north of Tehuantepec, usually grouped together under the name of Mexican Civilization, and those south of that isthmus, in the Mayan zone. For a long time before the Spanish conquest this entire region formed a definite cultural unit—it was a culture area, using this term in the precise meaning as defined by Kroeber[1]—for which the name Mesoamerica is now generally used.

Dr Kirchhoff, who has defined the cultural characteristics and limits of this area,[2] includes within it Northwestern Mexico, north to the Sinaloa River and the Pacific Coast of Central America, south to the Peninsula of Nicoya in Northwestern Costa Rica. In so doing, he is considering the situation which existed at the time of the discovery. From the point of view of the archaeologist—referring to the pre-Spanish past—I would prefer to include only the area roughly limited by the lines extending from the mouth of the Pánuco River to the mouth of Grande de Santiago River on the north, and from the mouth of the Ulúa River to the mouth of the Lempa River in the south.

The culture history of this area has been reconstructed for a period extending to over twenty centuries before the Spanish conquest,[3] through archaeological studies and, for the last centuries of this period, through native histories preserved either in the original form of folded books of picture writing, or in transcriptions made after the conquest—with the European alphabet in the Indian languages, or in Spanish—or in oral traditions written down in Colonial times.

Archaeology and, to some extent, Indian myths and history, tell of a profound crisis which shook Mesoamerican society and changed its character around A.D. 900[4]. The causes of this crisis are not yet known, but the consequences were unquestionably a change from a theocratic pattern to a secular-militaristic one, in which religion was still a

[1] *Cultural and Natural Areas of Native North America.*

[2] *Acta Americana*, Vol. I, No. 1.

[3] I am not considering here the San Juan and Chalco industries, for it is not possible as yet to link the ancient hunting and gathering cultures represented by those artifacts with the later development of Mesoamerican cultures. Radiocarbon dates for 'Archaic' (agricultural, ceramic) levels go back to the 15th century B.C. However, since the published list (1 September, 1950) includes dates so clearly conflicting with well-established stratigraphical sequences, I feel that RC dates for Mesoamerica need careful checking before we can use them confidently.

[4] In this paper all the dates earlier than A.D. 1000 are based on the Maya Long Count, using Thompson's correlation (11.16.0.0.0 L.C.: A.D. 1539). In the last few years there has been an increasing tendency to adopt a shorter correlation (11.3.0.0.0 L.C.: A.D. 1543) which would add 256 years to the dates here given. It is true that the evidence for the 11.16.0.0.0 correlation is not irrefutable, but still in his most recent book (*Maya Hieroglyphic Writing : Introduction*, 1950) Thompson reasserts, after reviewing all the other possibilities, that it seems to be the most acceptable. For later times in the Maya area, I also follow Thompson's chronology, as presented in his two excellent papers 'A Trial Survey of the Southern Maya Area' and 'A Survey of the Northern Maya Area', *American Antiquity*, IX, 1 (July 1943) and XI, 1 (July 1945).

powerful force of social control, but the priesthood was in a subordinate position to the temporal power, and there was a correlative change in the settlement pattern.

Evidence for the beginnings of this process, the development of the military class as a socially important force, exists from about A.D. 650, when figures of warriors and battle scenes began to be depicted in a prominent fashion on carved lintels and stelae and in mural paintings at Piedras Negras, Yaxchilán, and Bonampak in the Usumacinta Basin (Mayan area). Further evidence is found on some stelae at Calakmul (dated from A.D. 730), and Oxpemul (not dated, but recorded dates at this site run from A.D. 731 to 830) in Southeastern Campeche, where personages armed with a spear replace the figures of priests carrying peaceful religious symbols such as the manikin-sceptre and the two-headed serpent bar, which were formerly exclusively represented. For so early a date, no similar evidence has come to light as yet in Central Mexico.

During this period—from 650 to 900—Teotihuacán, the great metropolis of the Central Mexican plateau, was destroyed by fire and abandoned. Abandoned at about the same time—the close of the 9th century—were the great ceremonial centres which had been flourishing for some centuries everywhere in nuclear Mesoamerica—Monte Albán[5], Kaminaljuyú, Uaxactún, etc.

Before this general crisis, almost all known sites were situated in open ground, on plains or gently sloping hillsides, with obvious disregard for defence, and lacking—so far as is known—any artificial protective works. The famous ruins of Monte Albán are an exception to this, in that they are located on top of a hill 400 metres high, easily defensible. Nevertheless, twenty years of excavation have failed to produce any indication whatever that the site was selected with defensive purposes in mind, or that it was at any time used as a fortress. The character of this site was religious and funerary. As for the so-called ' Citadel ' of Teotihuacán—an imposing enclosure on flat ground—complete and very careful excavation, some thirty years ago, showed that it is nothing more than the precinct of a great temple.

Another—and this one seems to be a truly significant exception—is Becan, a site in the Rio Bec region of Southeastern Campeche and Southwestern Quintana Roo. The ceremonial centre is surrounded by a moat slightly over 1730 metres in circuit, 3 to 25 metres wide, and 2 to 4 metres deep, dug into the shallow soil and the underlying limestone, and interrupted by seven causeways, 3 to 4.5 metres in width, at irregular intervals[6]. The limestone is very porous and the moat obviously cannot have been filled with water, but as far as I know dry moats—possibly filled with thorny plants—were characteristic of Mesoamerican fortresses. Very possibly, though no excavation has been made to investigate this point, Becan's moat was a part of a fortification completed by a palisade, such as were used in the same region in later times, as, for example, in Cehaché. Had there been a stone wall its remains would be obvious.

No dates from this site have been deciphered, but on stylistic grounds we can fairly safely apply to Becan the dates recorded on the previously mentioned stelae from Calakmul and Oxpemul, sites which are very near. At present, Becan is the oldest fortified settlement I know in Mesoamerica, and it is significantly coincidental in location with the region in which the earliest representations of war and warriors are found. It is, however, necessary to keep in mind that, because of the present insufficiency of our knowledge, this entire picture could be changed by new discoveries.

The religious centre of Xochicalco (PLATE I), built on top of a steep hill and protected

[5] Subsequently used only as a burial ground.

[6] Ruppert and Denison, *Archaeological Reconnaissance in Campeche, Quintana Roo, and Peten.*

by a citadel, ramparts, and dry-moats, is the oldest fortified site known in Central Mexico. Although the precise date for the whole settlement, and for its different component parts, has not been conclusively ascertained, it would seem to be fairly safe to date it between the destruction of Teotihuacán and the beginnings of Tula, or about the 9th to the 10th

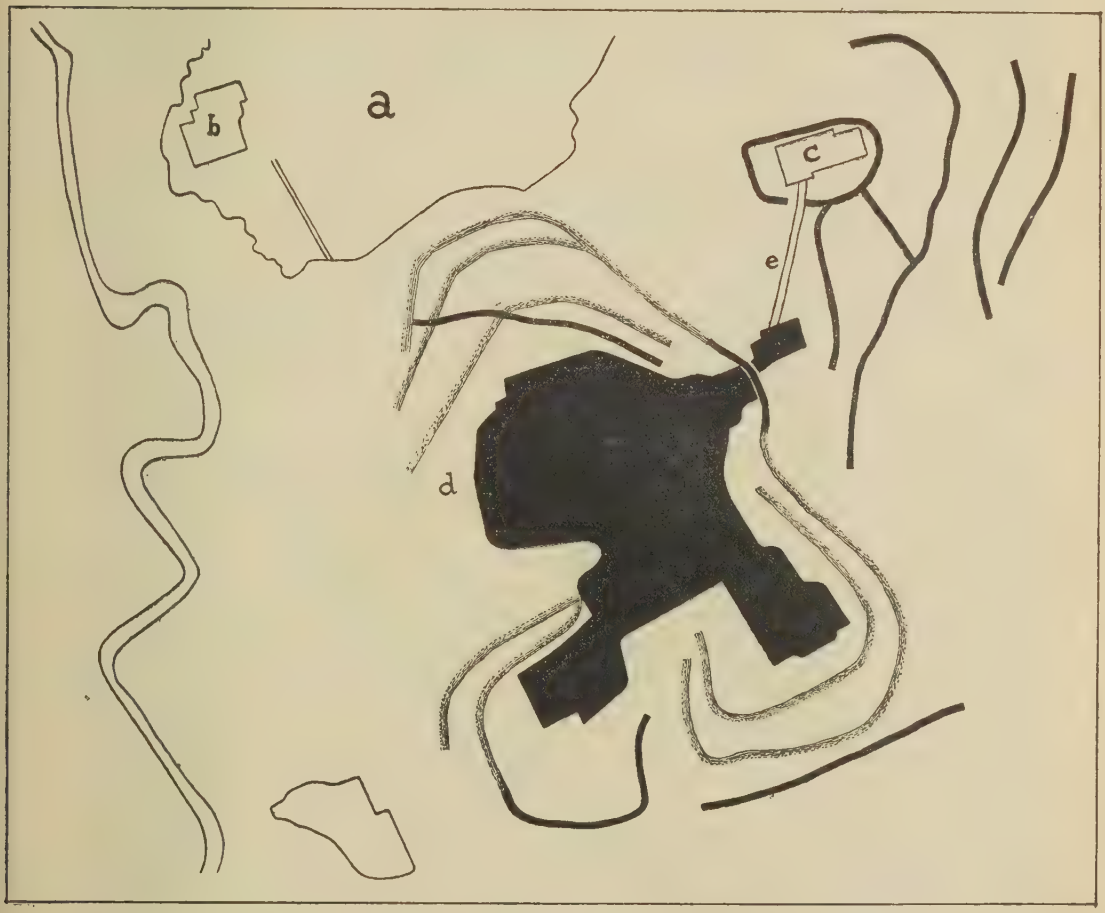


FIG. 1. SKETCH MAP OF XOCHICALCO (see PLATE I)
Black area, ceremonial centre; *heavy lines*, moats; *a*, habitation site; *b*, local temple; *c*, citadel; *d*, cemetery; *e*, paved way
Scale about 1 : 12,000. North at top

centuries. The site is composed of three different parts: (a) a religious centre on a steep hill, topped by the Temple of the Feathered Serpent, and surrounded by ramparts and moats; (b) a citadel on top of a neighbouring, higher hill, also surrounded by moats, and linked to the religious centre by a paved way lined by walls on each side; and (c) the habitation site on flat ground north of the mentioned hills, and protected by them and by deep ravines on the west and the north (FIG. 1).

After the crisis of the 9th and 10th centuries, many cities and towns established in open country were protected by stone-walls, or by palisades and moats. In other cases, only the *tecpan*, the section including the main temples and the palaces, was surrounded by a wall ; perhaps the idea of the walled *tecpan* may have developed from the ceremonial enclosures of the previous period. In addition, many settlements—with large permanent population—were hill-top sites, in most cases solidly fortified. In late pre-Spanish times, extended systems of frontier fortresses were built around the besieged territory of Tlaxcala, and along the border between the Mexican Empire and the Tarascan Kingdom. Also, isolated fortified hills were commonly used—in some regions—in wars involving petty lordships or tribal groups.

Assembling the available data on a regional basis, we find the following :

Central Mexico : This area included at the time of the Spanish conquest the heart of the Mexican Empire—the confederacy of Tenochtitlan, Tezcoco, and Tlacopan—as well as the enemy state of Tlaxcala. Tenochtitlan, the capital of the Empire, was protected by its location on an island in lake Tezcoco. The causeways, linking it to the mainland, were interrupted by gaps, spanned by wooden bridges which could be easily removed, as we know from the accounts of the Noche Triste, and of the final assault of 1521. Before the foundation of Tenochtitlan, when the Mexica were established on the hill of Chapultepec (at the end of the 13th century), they protected the settlement by successive lines of concentric stone walls.

The *tecpan* of Tezcoco was surrounded by a wall—the northern and western sides of which were of stone, and the remainder of mud-brick—varying in height from about 5 metres to over 8 metres. Still standing is part of a similar stone-wall in the neighbouring town of Huexotla, formerly a section of greater Tezcoco, which was a polynucleated city.

In Southern Puebla, in the rich valley of Atlixco—which was a very important irrigation area—the city of Cuauhquechollan (now Huaquechula) was surrounded with a masonry wall about 6.5 metres high, surmounted with a parapet and pierced by four entrances in which the overlapping inward turning ends of the walls formed defensible passages. In the same valley, the neighbouring town of Itzocan (now Izúcar Matamoros) was located on the high bank of a river and backed by a hill, which Cortés describes (1520) as a very good defensive position ; in addition, the town was surrounded by a parapet about 1.65 metres high. Also in Southern Puebla, there was Tepexi, a hill-fort site, located on a wedge-shaped mesa surrounded on two sides by deep ravines, and with the entrance protected by walls (still standing about 2 metres high) and, according to a description, moats, and a bastion with loopholes. About one kilometre from the entrance, there are remains which seem to be those of a detached fort. The surface pottery from the site indicates occupation during the time immediately before the Spanish conquest[7]. Another hill-fort site in Central Mexico was Teotenanco (now Tenango del Valle), in the Valley of Toluca—*tenanco* meaning in náhuatl ' walled site '.

Near the eastern Tlaxcalan border Cortés found in 1519, on top of a high hill and surrounded by a wall with barbican and moats, the very populous town of Iztacamaxtitlan. A short distance from the town, there was a frontier fortification built against the Tlaxcaltecs, consisting of a drystone wall extending from hill to hill across a valley. The wall was approximately 2.5 metres high and 6 metres thick, and was surmounted with a parapet; the only gateway in this wall was similar to those previously described for

[7] Chávez and Obregón de la Parra, unpublished MSS. in the files of the Instituto Nacional de Antropología e Historia, Mexico.

Cuauhquechollan. All around the territory of Tlaxcala there were—according to the historical sources—similar frontier fortifications. The fortress of Cacaxtla (s.w. Tlaxcala), in which five parallel moats cut into the rock forbade access to the hill on the side of the probable attack, may have been one strong point of this defensive system ; however, the origin of these defences goes back to the second half of the 12th century, and it seems that they were built by the Olmeca-Xicalanca before these people were expelled by the Chichimecs. The distance from moat to moat varies from 100 metres to 250 metres, providing thus an effective defence in depth. The width of the moats varies between

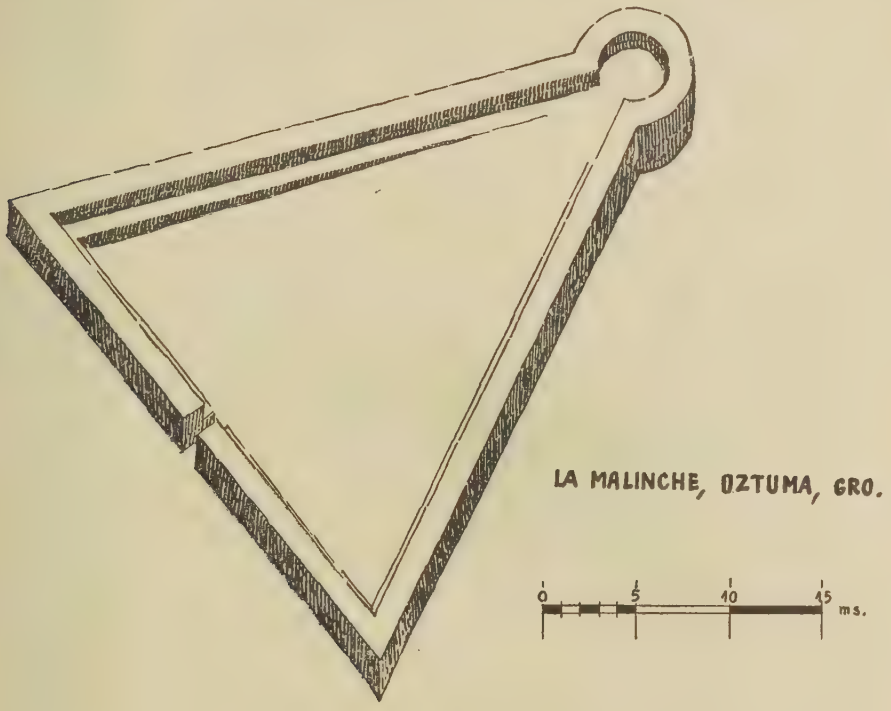


Fig. 2

9 metres and 24 metres, being in most of them about 13 metres, and the depth in some places is still today 6 metres. According to the 16th century historian Diego Muñoz Camargo, there were walls supplementing the moats.

After the 1470's, the major enemy of the Mexican Empire was the Tarascan Kingdom of Michoacán, to the west. The Mexicans were waging a defensive war on this front at the time of the arrival of the Spaniards, after some unsuccessful attempts to invade the enemy's territory, which ended in bloody defeats. Lines of forts and garrisons were maintained along the frontier on both sides, and in the no-man's-land between them minor clashes were frequent.

The southern section of the Mexican line, running across the southern escarpment of the Central Mexican plateau, south to the Balsas River, was commanded by the

virtually impregnable fortress of Oztuma, built after the reconquest, by the Mexicans in the late 1480's, of the Chontal province which had revolted. The main fort was established on top of a high hill with precipitous sides and with only a very narrow entrance, defended by moats and walls surmounted by parapets. This narrow entrance leads to another hill, topped by a small fort triangular in shape (FIG. 2) and surrounded by a stone-wall which follows the contours of the upper slopes of the hill. On the long sloping spurs of this hill there was a system of parallel parapets extending from ravine to ravine. The distance between the outermost wall and the main fort is about three miles. Notwithstanding the depth of the defences, the Tarascans succeeded in storming their way through all the outer walls, and were besieging the main fort in 1519.

About ten miles north of Oztuma, there was (near Alahuiztlan) another fortified hill, belonging to the command of Oztuma. This fort is described as a rounded hill girt by three concentric walls, each of them with its moat. It was built for the purpose of protecting the salt deposits of Iztapa and Tzicaputzalco.

The Chontal town of Oztuma, which was stormed by the Mexican king Ahuitzotl about 1487, was located some four miles southeastward from the hill on which the Mexicans afterwards built the main fortress. It also was situated on top of a steep hill and was girt with stone-walls and trenches.

The Southern Sierra Madre Region: includes the modern states of Guerrero and Oaxaca. With the exception of the Zapotecan Kingdom of Tehuantepec and the important Mixtecan Lordship of Tototepec on the Pacific Coast, this area was politically fragmented into rather small lordships engaged in frequent local wars until they became the prey of the expanding Mexican Empire. Only the Lordship of Tototepec and the territory of the fierce Yope had escaped Mexican conquest.

A very important town in the Northern Mixtecan zone was Coaixtlahuacan, subdued by the Mexican King Moteczuma Ilhuicamina some time between 1458 and 1461. The present town is built on the ruins of a pre-Spanish settlement and there is a 16th century church on top of an Indian pyramid. To the west of the town there is Inguiteria, which was a settlement on a mesa 150 metres high, surrounded by ravines; it covers over 25 acres. There is no indication whatsoever of any occupation of the site before the crisis of the 9th century, yet there are a few indications of occupation shortly afterwards. Nevertheless the bulk of the remains on the mesa belong to a still later period, and evidence has been found that the site was still occupied after the Moteczuma Ilhuicamina's conquest[8].

Near the Pacific, in Southeastern Guerrero, there were the walled towns of Tototepec[9] and Quetzaltepec, which were attacked by the whole armies of the Mexican Empire in 1515. The main wall of Tototepec—there were five—was about 10 metres high and about 6.5 metres thick. Indian history tells us that 20,000 men worked building the fortifications, and that the approaches were protected by road-blocks made of logs, stones, and thorny brush. The walls were built of stone set in mud and strengthened with logs and fascines. The defences of Quetzaltepec were similar, and there were stone stairways on the inner side of the wall to give access to the parapet. The narrative of the storming of Quetzaltepec by the troops of Moteczuma Xocoyotzin

[8] Bernal, *Revista Mexicana de Estudios Antropológicos*, tomo X.

[9] Not to be confused—although both of them are in the same general region—with the Tototepec mentioned above, as I wrongly inferred in a previous paper. Barlow has made clear that the Tototepec stormed by Moteczuma Xocoyotzin, was a Yope border town located near present-day Ayutla. See *The Extent of the Empire of the Culhua Mexica*.

is very illustrative of the native technique of attack and defence of strongholds, which involved a very dynamic concept of defence. First the defenders deployed troops in front of the walls, in order to prevent the assault or sapping of the walls. When these troops were thrown back by the attacking forces to the base of the wall, they were under the protection of the missiles thrown by the warriors stationed behind the parapets. Thus protected and able to reorganize, they could counter-attack. For two days they repelled their assailants, but the third day the Mexicans reached the outer wall and stormed it using ladders they had ready and undermining the foundations to open breaches. To break through the successive walls required new assaults.

Tetenanco, near Tlapa, was another hill-top settlement. The name means ' place of the stone-wall ' and it figures in the Codex Mendoza (folio 39r) as a wall with merlons over the glyph for stones.

Fig. 3. NATURAL REGIONS OF MEXICO
By courtesy of the Royal Geographical Society

We are told by 16th century reports that elsewhere in this region, in pre-Spanish times, the most common practice of war was to fortify isolated hills with stone parapets and ditches ; this being done, both by the Mixtecs (Petlalcingo, Teozacoalco, Texopan), and the Zapotecs (Nexapa and Mitla). A 16th century map of Texopan depicts a conical hill, which was fortified with three concentric stone walls. A wooden drawbridge was part of one of such fortifications, near Nexapa. Still standing are the concentric walls of one of the forts near Mitla.

The Uplands of Chiapas and Guatemala. This region was divided into rather small political units, based, it seems, on ethnic affiliations. However, this ethnic basis appears to be of small significance, because of the close affinities of all the peoples.

Around 1500, the great Mexican conqueror Ahuitzotl waged a campaign in the eastern territory—Highland Chiapas—which resulted in the establishment of some garrisons, but not in effective political domination ; the Mexicans did not push beyond the present Guatemalan frontier. About a quarter of a century later the Spanish

Conquistadores found it necessary to attack the Tzotzil town of Chamula, which was fortified with walls and ditches. Bernal Díaz del Castillo describes in detail the storming of Chamula, and mentions that there was in the same region the town of Güeyguiztlan (i.e. Hueyhuitztlan, probably modern Huixtán) which was composed of three hill-fort sites.

For Highland Guatemala, Thompson[10] has pointed out that the sites known to be of recent origin (after A.D. 1000) or suspected of being late, are for the most part situated on mesas surrounded by ravines, approachable in most cases only across narrow necks ef land. He mentions Zaculeu, stronghold of the Mam[11]; Utatlán, capital of the Quiché; Iximché, capital of the Cakchiquel; Rabinal; Río Blanco; and Chutix Tiox (PLATE II). The defences of some of these places were strengthened by stone walls. This settlement pattern is in marked contrast with settlement on open ground, which was prevalent in this region prior to A.D. 1000, as everywhere in Mesoamerica, and it is attributed by Thompson to the arrival (known from traditional sources) of warlike Central Mexican people, as a result of the Toltec expansion.

Honduras. In Southwestern Honduras there was a mesa-settlement known at present by the name of El Rincón del Jicaque. The site is bounded by the Lempa River, and a crude stone wall closes the narrow entrance to the mesa. Strömsvik and Longyear[12] consider it rather a place of refuge for the people of the surrounding region in troubled times than a permanent settlement. They believe that it is a recent site, probably occupied during the period immediately preceding and contemporary with the Spanish conquest. We are told by the chronicles that the chief Copán Calel offered the last resistance to the Spaniards (1530) in a similar stronghold. Other hillforts in this region were Coyocutena and the Peñón de Cerquín, last refuge of the chief Lempira.

Farther east, in the Comayagua Valley, there is the mesa-settlement of Tenampúa, which seems to have been occupied at the time of the Spanish conquest.

Lowland Maya. In Yucatán the last centuries before the Spanish conquest witnessed the rise and decline of Toltec influence, in the same way as in the Guatemalan Highlands. Some degree of political unification was attained first under the hegemony of Chichén Itzá (987–1204), and later under that of Mayapán (1204 - *ca.* 1460). After the fall of Mayapán came the rise of some eighteen independent warring lordships.

The city of Mayapán, which occupied an area of over two square miles, was protected by two dry-stone walls. The inner one was 600 to 700 metres in circuit, and enclosed only the main temples and the palaces of the nobles. The outer wall was almost 9 kilometres in circuit, and had stairways which gave access to the parapet from the interior; it enclosed the habitation area of the commoners. The inner wall had two gateways, and the outer one, nine.

Chichén Itzá also—or at least the main section with the temples and palaces—was enclosed by a stone wall 4 metres thick, and preserved today to a height of 1.5 metres, but it must have been higher. Recently one of the gateways of this wall was excavated. It had a double arch and there was a guard-room situated between the arches[13].

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[10] *American Antiquity*, IX, 1, pp. 124–8.

[11] However, the origin of this settlement—as a religious centre, at least—contrary to previous assumptions is dated by recent excavations to nearly 1000 years before the Spanish Conquest. See Woodbury, *American Antiquity*, XIV.

[12] Carnegie Institution of Washington, *Notes on Middle American Archaeology and Ethnology*, No. 68.

[13] Ruz, unpublished MS., Instituto Nacional de Antropología e Historia, Mexico.

Other walled sites of this period were Chacchob, whose walls are still standing to a height of 6 metres; Aké, where there are moats and remains of a wall; Champotón, which at the time of the discovery was encircled with a dry-stone wall and fosses; and Ichpaatún. The wall of Tulum surrounds the site on three sides, and the fourth is formed by the cliffs overlooking the Caribbean Sea (PLATE III). The wall is pierced by five postern gates which also have guard-rooms. Not far from Tulum, and near Xelhá, there is a small peninsula which was fortified with a stone wall across the isthmus.

The area of Central and Eastern Tabasco, Southern Campeche, and Northwestern Petén was also politically fragmented. In this region, the town-walls were made, as a general rule, of heavy timbers. The Maya name for this type of defences was *tulumché*. The most elaborate type known is that of the place miscalled by the Spaniards, Cehaché—a name which seems to have been used by the Indians for the entire province—and described by Cortés in the account of his journey from Mexico to Spanish Honduras (1525). The approaches had pitfalls with pointed stakes at the bottom. The town itself was established on a small rocky hill, protected by a lagoon on one side and a small river on the other. Encircling the town there was a deep moat, supplemented by a timber breastwork, and behind this was a wooden wall about 3.5 metres high, with towers at intervals 6 metres high. This wall and the towers had loopholes. Not far away was the town of Tiac, which was located on flat ground, and therefore defended by even heavier fortifications. It was composed of three wards, each one defended by a palisade similar to that described for Cehaché, and there was another stockade common to all.

Other palisaded towns are mentioned: one near the mouth of the Grijalva River, attacked by the Spaniards in 1519; Cimatán, farther upstream; another found in Southern Campeche (probably near Matamoros) by Alonso de Avila in his expedition of 1530[14]; and, farther north, the town of Campeche. Furthermore, palisades were commonly used in Yucatán as road-blocks.

THE GULF COAST. Narratives of the campaign waged by Moteczuma Ilhuicamina against the Cuetlaxtlan region of Central Veracruz[15] tell us of fortifications consisting of moats, walls, and towers. However, it seems that the battles of this war were fought on open ground.

In the Totonac region many towns were established on mesas or rocky cliffs. Thus Xicochimalco, Quiahuiztlan, and Tizapantzinco are described as hill-forts by the Conquistadores Cortés and Bernal Díaz, and others, such as Tuzapan, Metlaltoyuca, and Rincón de Moteczuma are known archaeologically. In the escarpment of the Mexican Plateau, Cuauhchinanco (now Huauchinango) was a palisaded town; the name in náhuatl means 'the place of the wooden walls'.

We are told by the chronicles that farther north, in the Southern Huaxtecan region, in order to resist the Mexican invasion (*ca.* 1451), all the menaced towns had been fortified with five successive walls[16].

WESTERN MEXICO includes the modern states of Michoacán, Jalisco, Colima, Nayarit, and parts of Guanajuato and Zacatecas. The largest political unit of this region was the Tarascan Kingdom of Michoacán, which during the 15th century had grown strong

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[14] Scholes and Roys, *The Maya Chontal Indians of Acalan-Tixchel*, p. 70, quoting Oviedo.

[15] Tezozomoc, *Crónica Mexicana*, ch. XXXII. This campaign is dated by Barlow 1461–2; see *Journal de la Société des Américanistes*, XXXVI, p. 218.

[16] Tzicoac, Tamapachco, and Tochpan, are mentioned in the account of this campaign. See Tezozomoc, ch. XXVIII, and Durán, *Historia de las Indias*, ch. XIX.

enough to check the Mexican expansion, to repel the attempts of invasion, and to invade in turn the territory of the Empire.

The Tarascan frontiers were guarded by garrisons strategically placed. Taximaroa, facing to the Mexican frontier, was the seat of one of these garrisons. It is described as a large town encircled by a strong palisade over 3.5 metres high and 1.8 metres thick[17]. Information is lacking about the type of other frontier strongholds.

In the region of the Lower Lerma and Santiago River Valleys rocky hillocks were hastily fortified with stone breastworks when necessary[18].

In Southern Zacatecas, on the boundaries of Mesoamerica, there was the rugged territory occupied by the warlike Cazcans (or Caxcans). It seems that most of the Cazcan settlements were on top of mesas with narrow entrances, further protected by stone breastworks. Thus, Miztón, Nochiztlán, El Teul, etc.[19] This refers to the situation in the early 16th century. In the northern fringe of this territory there are the ruins of La Quemada, a site protected by an acropolis fortified with dry-stone bastions (PLATE IV). La Quemada had been destroyed by fire and abandoned before the arrival of the Spaniards. No Indian tradition has been preserved illustrating its age, and the little archaeological work done there has failed to give a clue to its date.

Farther northwest, in the Sub-Mesoamerican area of the Sinaloan coastal plain, some villages, such as Piaztla and Ciguini, were encircled by palisades. But inland, at Topía, situated in the rugged Western Sierra Madre, stone walls and cacti were used for fortifications[20].

Summarising the data available for late pre-Spanish times we find :

(*a*) palisades had a split distribution. To the east, in the Lowland Maya region and Huauchinango ; to the west, Taximaroa and the Sinaloan coastal plain.

(*b*) hill-top and mesa settlements were frequent in all the mountain districts, and to fortify isolated hills was a common practice of war.

(*c*) walled cities (or sections of cities) on level ground are known from Central Mexico and Yucatán.

(*d*) extended systems of frontier fortifications coincided with the larger and more integrated political units : the Mexican Empire, Tlaxcala, and Michoacan.

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[17] Herrera, *Historia General de las Indias*, Decade III, bk. III, ch. III.

[18] Tello, *Crónica Miscelánea*, pp. 93, 444, 467.

[19] Tello, pp. 19, 20, 90–2, 367, 447, 457. Baltasar de Obregón, *Historia de los descubrimientos* . . ., pp. 31, 36, 37. García Icazbalceta, *Colección de documentos* . . ., Vol. II, pp. 270–1, 309, 343.

[20] Baltasar de Obregón, p. 58.

Because of limitations of space, reference has been made only to material, published after, or unknown to me at the time of the publication of my previous paper on this subject. For the remaining sources, see ' Fortalezas Mexicanas ', *Cuadernos Americanos*, 1948 : 5.

The Kensington Stone

by Erik Moltke

Runologist of the Danish National Museum

Translated by John R. B. Gosney

In 1898 a Swedish-American farmer, Olaf Ohman, cut down an aspen tree on his farm. He found enveloped in its roots a large flat stone which he only just managed to get out without ruining his axe. When his ten year old son had brushed some of the dirt from the stone, Ohman discovered that one of the faces and one of the edges were covered with strange engraved figures. It was soon decided that these had something to do with runes. This is where the saga of the Kensington stone begins : from Ohman's farm to a shop window in Kensington; to Prof. O. J. Breda in Minneapolis, to Prof. George O. Curme, back to Ohman's farm condemned as a blatant forgery. Here it lay despised as ' a stepping stone near his granary for eight years, without further notice '. It was ' rediscovered ' by Hjalmar Holand, bought by him, and he devoted his life (three large books and innumerable articles) to attempting to prove that the inscription was genuine. Finally in its Jubilee year 1948 it was given the place of honour in the National Museum at the Smithsonian Institute in Washington as one of the finest pre-Columbian monuments of America.

American and European newspapers during the Jubilee year were full of articles about the stone, partly because two leading Scandinavian scientists had been called to America to give an opinion. They were the Swedish runologist Sven B. F. Jansson, who declared subsequently in a talk on the American radio that the stone was false, and the Danish professor of Archaeology Johs. Brøndsted, who brought back an excellent copy of the inscription but otherwise left the conclusive word to the students of runes and language.

In 1910 a little book was published :—The Kensington Rune Stone : Preliminary Report to the Minnesota Historical Society by its Museum Committee. The result of the investigations by the committee was that the inscription was genuine, pre-Columbian. Nevertheless the report ends with a letter from Prof. N. H. Winchell, the philological expert on the committee. In it he is sorry that he is going away, but he says : ' I have examined your report carefully, have visited Kensington and neighbourhood, and have read most of the papers and articles relating to the rune stone. I have always agreed with the great authorities of Norway and Sweden, Magnus Olsen, Moltke, Moe, M. Hogstad (i.e. Haegstad), Bugge, Noreen, Schrick (i.e. Schück), Montelius, in thinking that the language is too modern, besides being faulty ; and a more careful study of the words has not changed my opinion '. To these outstanding names, and especially the runologists and philologists Sophus Bugge, Magnus Olsen and Adolf Noreen, may be added the names of the greatest runologists of Sweden and Denmark, Otto v. Friesen, Elias Wessen and Ludvig F. A. Wimmer. Now two younger runologists have severely questioned the authenticity of the stone, namely the present writer[1], and also Sven B. F.

[1] In the newspaper *Information*, of 9th Feb. 1949, on the basis of the material brought back by Brøndsted, later published in the journal *Danske Studier*, 1949–50, with a linguistic examination of the inscription by Dr Harry Andersen.

Jansson in *Nordisk Tidskrift* 1949. A couple of years before the appearance of these articles the above-mentioned journal *Danske Studier* for 1946–47 published an article on the inscription by the Danish Emeritus Prof. in the Eskimo Language, William Thalbitzer, who upholds the authenticity of the inscription. Recently a paper by S. N. Hagen has appeared in the American Journal *Speculum* (July 1950). Hagen says: 'As far as I know, no linguist or runologist has come forward with the reconsideration suggested by Einarsson [disagreeing with Holand who is deficient in certain elementary fundamentals]. While we are awaiting a study of the inscription by a competent scholar, I offer a few observations . . .' Hagen supports the authenticity of the inscription, for example in the following words: 'This inscription should be a perfect joy to the linguist because it is such a delightfully honest and unsophisticated record of its author's own speech. A forger would have tried to imitate a language other than his own. It is clear that this author tried to imitate no language but his own. In branding this beautiful inscription as a forgery, scholars have thrown away not only an important historical document but also a faithful record of medieval Scandinavian speech'. Finally I am acquainted with an article in the Swiss Journal *Atlantis* (Sept. 1950). It is written by one of the former supporters of the inscription, Prof. Richard Hennig, Düsseldorf, and although he knows both Jansson's and my article, he concludes his statement with the following pompous words, reminiscent of the runologists of the Hitler period: 'The authenticity of the Kensington Stone has been proved and thereby the presence of Scandinavians in America fully 130 years before Columbus is no longer in doubt[2]'.

A far as the most recent authors are concerned, one may well say that Thalbitzer's philological arguments have been so hard hit by the specialised criticism of Jansson and Harry Andersen that they have no leg to stand on. In spite of Hennig's determined judgment he produces no new argument, but confines himself to an enumeration of all Holand's old chestnuts. As for Hagen, his article shows that he is a scholarly Scandinavist—from time to time he puts his comrade in arms, Holand, gently and kindly on the right road, when the latter displays his ignorance of the elementary rules of Old Nordic grammar. He makes a really honest attempt to confute the philological arguments against its authenticity, but his deep attachment to 'this beautiful inscription' gives its peculiar forms of speech such a wide margin as to leave the thinking reader with the impression that had the inscription been in Chinese, Hagen would have let it pass as good Latin! But more of this below.

It is striking not only to the Scandinavian scholar, but to any ordinary reader of the Kensington inscription that this living document can be very easily read when transcribed in the Latin alphabet. But this is not the impression one gets from the language of the 14th century which is somewhat incomprehensible for the ordinary Scandinavian reader. That is why Hagen looks in vain for writings by Scandinavian specialists about this stone. They do not merely believe and feel that this inscription is impossible, but know it for certain in the same way as an Englishman would know at once that an inscription in modern English with a few old-fashioned forms added (which moreover combine words of different gender and case) could not belong to the 14th century. To show how *every* Scandinavian scholar regards the inscription I will quote what Professor Jon Helgason, professor in Icelandic at the University of Copenhagen, said to me when he read my first article on the Kensington Stone: 'In my opinion the inscription on the

[2] German original: 'Die Echtheit des Kensington-Steins ist erwiesen und damit die Anwesenheit von Skandinaviern in Amerika volle 130 Jahre vor Columbus nicht mehr zu bezweifeln'.

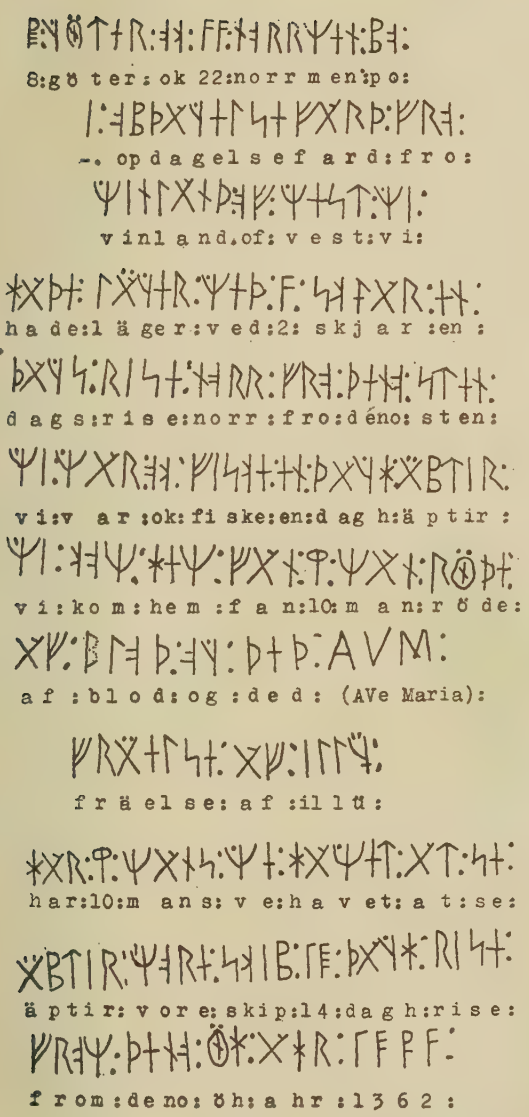

8:g ö t e r : o k 22:n o r r m e n:p o:
op d a g e l s e f a r d:f r o:
v i n l a n d.of: v e s t:v i:
h a d e:l ä g e r:v e d:2: s k j a r :e n :
d a g s:r i s e:n o r r :f r o:d éno: s t e n:
v i:v a r :ok: f i s k e:e n:d a g h:ä p t i r :
v i : k o m : h e m :f a n:10: m a n:r ö d e:
a f : b l o d:o g :d e d : (AVe Maria):
f r ä e l s e: a f :i l l ü :
h a r:10:m a n s: v e:h a v e t: a t:s e:
ä p t i r: v o r e: s k i p:14:d a g h:r i s e:
f r o m :d e n o: ö h: a h r :1 3 6 2 :

Fig. 1. THE INSCRIPTION ON THE KENSINGTON STONE

Translation

(We were) 8 Goths and 22 Norwegians on a journey of exploration from Vinland to the west. We had (our) camp by two skerries one day's journey from this stone. We were out to fish (fishing) one day. Arrived home we found 10 men red with blood and dead. A V M (AVe Maria), free (us) from evil. (We) have ten men by the sea to look after our ship(s) 14 days' journey from this island. (In the) year 1362.

Kensington Stone is such that no philologist with any self-respect could in any decency write about it ; any more than an archaeologist would trouble to publish a grave-find of the Iron Age if he found a telephone book under the urn '. In my heart of hearts I agree with Jon Helgason. On the other hand there has been so much fuss made about this inscription that a stop must be put to it.

In the language of the 14th century we expect to find, e.g., ' *wi hafdhum* ' instead of the modern ' *wi hade* ' (we had), which is on the stone, or *wi varum, komum, funnum* instead of the stone's *wi var, kom, fann.* In general we expect to find inflected endings in both verbs and nouns. However, we do find—quite sporadically, it is true—entirely modern uninflected forms as well (e.g. ' *kom* ' for ' *komum* '—we came, etc.). The only 14th century inscription which shows complete consistency in this matter is—that on the Kensington Stone.

There are a few other small details which—to put it mildly—are offensive to the ears of a Scandinavian philologist, and I shall mention them because Hagen has now made a noble attempt to justify them. If the interested reader takes a look at the copy of the inscription he will find that ' from this island ' is ' *from þeno öh* ', in another place he finds that ' from ' is also ' *fro* '. Hagen writes : ' In an older stage of the inscriber's dialect both *frā* and *fram* (with short *a*) must have existed side by side ; but since both forms had exactly the same meaning, *fram* must (!) have yielded to pressure from the more dominant (!) *frā*, thus becoming *frām.* When *ā* became open *ō*, *frām* would have become (*frɔ:m*) just as ' *frā* became (*frɔ:*) '. This argument is really strikingly effective also because the same sound shift did *not* take place in the word *skjar* (which Hagen translates by shed) and *ahr* (year), just as the development *frăm* > *frām* > *frōm* > *frŏm* must be designated as striking. As far as *þeno* is concerned (this), it is the only inflected word in the inscription. Unfortunately this form, which is a (newly developed) form of the neuter in the dative singular, here qualifies a word (uninflected) (*öh*) which is feminine, and elsewhere in the inscription is connected with a masculine word *þeno sten* (which is nevertheless not in the dative) ; in genuine 14th century language these combinations would be : *þessum steini* and *þessi eyju.* Finally the last of the three small words is completely impossible in the language of the 14th century, *öh*, with *h* to indicate length, since this phenomenon is considerably more recent than the 14th century. Hagen is extremely off the track when he tries to explain this *h* as the best proof that the inscriber of the stone was a specially fine phonetician, since this word in Danish certainly, but never in Norwegian or Swedish, had an ' aspirated off-glide '.

We could keep on like this ; but we will spare the now impatient reader further philological explanations. Let us pretend that the language is in order ; without a qualm we will take it for granted that the inscriber, who, as the diagrams show, knew both the runic alphabet and the Latin alphabet (to some extent), has forgotten everything he learned at his monastic school about the rules of spelling which he must have known at one time, and that he wrote as he spoke. This is what the supporters of the inscription would have us do, and we will forget that this supposition is not able to explain the linguistic anomalies either, even if we are willing to admit that our knowledge of the spoken language of the 14th century is very slight. We admit that language is a strangely lively fish which it is given to few armchair philologists to grasp. We will for the time being turn our backs on the language and consider the runes, to see if the solution to the problem may not lie with them.

Runes ceased to be used in Denmark and Norway about 1300, but they survived in Sweden on grave-stones and on household goods, not to mention the runic calendars which continued up to the 1700's. But it is actually true of all medieval runic inscriptions

whether of Denmark, Norway or Sweden, that there is an even development to be traced from the earliest Viking times right up to the latest runic inscription, not only in the forms of the runes themselves but also in the language. The Kensington inscription does not fit into the unbroken chain of Scandinavian runic inscriptions. Look at the alphabets and see for yourself that almost half the stone's runes have shapes which are not to be found in the runic alphabet of the Middle Ages.

Fig. 2. THE USUAL SCANDINAVIAN RUNIC ALPHABET IN THE MIDDLE AGES

It is however a fact that runes, as mentioned above, were retained right down to the 18th century in the Swedish runic calendars in the possession of the common man in many Swedish districts. And if we examine these we find that those of the 16th and 17th centuries employ very degenerate forms of the runic alphabet—forms which are not unlike those on the Kensington Stone. Therefore there can be no doubt that the person who engraved the Kensington Stone constructed his alphabet on the basis of the alphabet of such a Swedish runic calendar, and this is completely corroborated by the symbols which he used for numbers. They are those usual in runic calendars (known since the 14th century) but completely unknown in the general run of runic inscriptions. The engraver has not slavishly followed the alphabet of his runic calendar but has invented new symbols. In the formation of these new symbols he has offended against the system of the runic alphabet in ways we shall not go into here. The patient reader interested in the Kensington Stone will have already noticed that it is now in rather a precarious position. But it has not received the coup de grâce. Here it comes. In his eagerness to have as complete an alphabet as possible the engraver of the Kensington Stone has invented a j-rune. He ought not to have gone as far as that. The fact is that the letter ' j ' is a development within the Latin alphabet (like v). Both these letters were invented by the French philosopher Petrus Ramus in the 16th century. He took the letters *jod* and *vau* from the Hebrew alphabet and supplied the Latin alphabet with these two sadly needed consonants. In Scandinavian and German *jod* kept its name, while *vau* became *vee* in Scandinavian and English. Only the Germans have kept the old Hebraic name *vau* (*fau*)[3]. Just as striking is the letter ö, with the two dots above it, on the stone. This trick was introduced into Sweden about the time of the Reformation. In other words we have before us a rune-stone which used symbols—j and ö—which were not invented until *c.* 1550, and the stone is dated 1366 !

Now all the linguistic objections are on a firm footing ; now we really understand the forms like *vi hade* instead of *wi hafdum* ; now we realise how a word like *opdagelses-faerd* ', impossible in the 14th century, can be found in the inscription (*opdage*, discover,

[3] This j is one of the details that exposes Holand's lack of knowledge in most elementary things, since he is pleased to refer to a couple of Norwegian diplomas which in his opinion show many examples of j. These are in fact not j's but long i's which are known to have been used from the Middle Ages up to more recent times as a sort of graphic (but not phonetic) variation, a principle which is shown for example in such discoveries as ' vi ' (we) written ' wij ', or the Roman numeral viii (VIII) as viij, without anybody, who has some knowledge of old writing, dreaming of talking about a letter j (a consonant—or semi-vowel) without a distinct sound value.

meant *oplyse*, enlighten). With our eye on the American language we nod in recognition to words like *ded*[4] (dead) and *from*.

But it is possible that there may be still a sceptic maintaining that Petrus Ramus—by thought transference—got the idea for the letter j from the author of the Kensington Stone inscription. Leading mineralogists have really accounted for the weathering and patina of the stone. But Professor Brøndsted's examination has a crushing reply to this. He has proved that an h carved in the stone by Holand about 40 years ago has already taken on a certain patina. And that in spite of the fact that in these 40 years the stone has not been acted upon by the wind and weather but has been kept in a sheltered room. Farewell, Kensington Stone of 1362, farewell Paul Knutsson expedition,[5] which perhaps never even started and which very likely never got to America; at any rate farewell to all the fruitless labour of scholars of nearly every branch of learning.

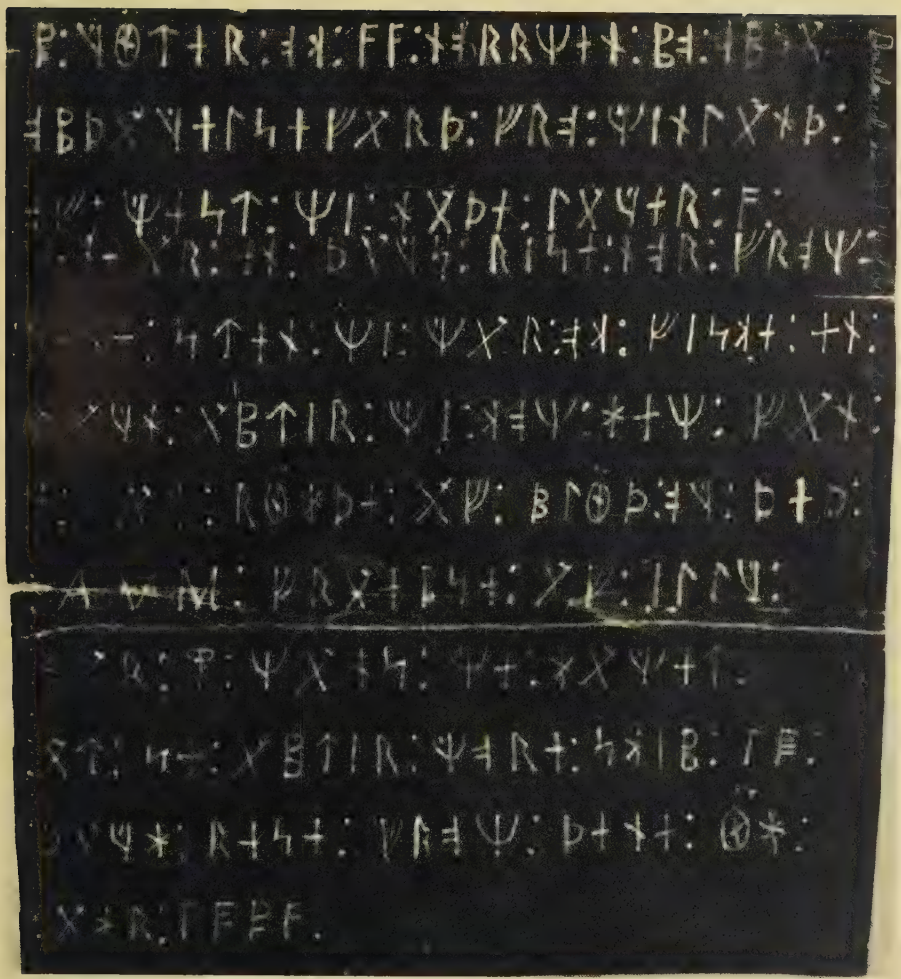
Apart from the battering Sven B. F. Jansson gave the Kensington Stone inscription both in general and in detail in his above-mentioned article, it is interesting to note that he shows how Holand in his massive books seems to have suppressed important material which argues against the authenticity of the stone.

From information I have received from Prof. J. A. Holvik, Moorhead, Minnesota, it appears that not a single archive of the Minnesota Society was properly examined by Holand. Prof. Holvik here discovered a document which must be considered as no less than sensational in this connection. It concerns a sheet of paper covered with runes and is apparently the *engraver's rough draft for the inscription on the Kensington Stone.* Holvik, who published it in the paper *The Concordian*, no. 10, Nov. 18, 1949, accompanies it with the following words: 'This sketch of the Runestone inscription was sent in a letter by J. P. Hedberg of Kensington, to Swan (Swen ?) J. Turnblad of Minneapolis. The letter is dated January 1, 1899. Mr Hedberg calls the sketch an exact copy of writing on a stone brought to him by Olof Ohman. Both the letter and the sketch were filed in the archives of the Minnesota Historical Society in August 1925. (Holand's first book on the inscription was published 1932). I found it there last month. A detailed comparison of the individual characters and spelling of each word with those on the Kensington stone shows that this sketch is not a copy of the inscription on the stone. *It is an original preliminary draft of the story later inscribed on the stone*'. With reference to PLATE V, which is a reproduction of a photostat copy sent to me by Holvik, I emphasize in agreement with him that (1) in PLATE V the word 'from' is written *fro* in the first line and *from* in the fourth; the stone has here only *fro*. (2) PLATE V has the word 'red' written with h-rune (*røhde*). The stone has *røde*. (3) PLATE V spells the word 'blood' as *blød*, the stone has correctly *blod*.

The sketch is older than 1899; at some time before this year (and no doubt in the autumn of 1898, when the stone was found), it was given to a man (Hedberg) by the finder of the rune stone (Ohman), who publicly had declared that he knew nothing about

[4] Hagen attempts to explain these words plausibly, but unsuccessfully.

[5] Holand succeeded in giving a sort of historical background to the inscription. He produced a letter from the Swedish king Magnus Eriksson dated 1354. In this the king commands the Norwegian noble Paul Knutsson to fit out an expedition to Greenland, there to restore declining Christianity. Holand now imagines that the Paul Knutsson expedition arrived in Greenland, but, not finding the Norsemen there because they had either been killed off by the Eskimos or else had emigrated to Vinland, took the expedition to Vinland (America) to find the backsliders and drag them back to the Kingdom of God by the hair. Hence the inscription on the Kensington Stone is a record of what happened to some members of the expedition and a warning of what Fate had in store for the rest.



THE HEDBERG-OHMAN DRAFT OF THE INSCRIPTION

a. PARTIALLY RUINED *BORIE* (note small window)

b. LARGE *BORIE* NEAR GORDES

ROMAN CAMPS AT HILLSIDE, NEAR DUNBLANE, PERTHSHIRE
ph. J. K. St. Joseph. Copyright reserved

PLATE VIII

SHABAKO SCARAB
FROM SENNAR

runes. Nothing is intimated of how far the sketch was undertaken by Ohman. But the spelling of *røhde* compared with *røde* (on the stone) cannot have been a transcriber's error by a man who knew nothing about runes, and that is evidence that the sketch is not a copy of the stone's inscription. This is stressed and strengthened by the two other examples. Notice also that *þeþ* (dead) was first written *þöþ*, but corrected to *þeþ*. No reader, whether layman or not, could transcribe the stone's regular and correct o-rune in *blod* by the complicated ö-rune. But if the paper is not the copy, then it must be the original!

In this connection I must draw attention to a book, some of whose pages have been copied photostatically and the copies sent to me by Prof. Holvik. These are extremely interesting. The Book is called: *Den kunskapsrike Skolmästaren eller Hufvedgrunderna uti de för ett borgerligt samfundsliv nödigaste Vetenskapen*, by Carl Rosander (The well-informed Schoolmaster or the Fundamentals of popular Science), 1883, new edition 1893. On the title page is Ohman's signature and 'Kensington 2.3.91'. On pages 63 ff is to be found an account of the Swedish language and its development. An example of the Lord's Prayer of *c.* 1300 ends with the words: *frælsæ os af illu*; here can be found the spelling *ok* and *og* (and), here even can be found *h* to lengthen vowels, and remarkably enough the spelling *röhd* (red).

How these remarkable coincidences are to be explained is naturally difficult to say. But it must be admitted that the consensus of opinion among Scandinavian scientists is that if there was in America at the end of the last century a Scandinavian with no training in philology but who had dabbled a little in books on popular science, and if he had had the idea of making a runic inscription, then that inscription would take on the same appearance as the one on the Kensington Stone.

Notes and News

FALSE-VAULTED STONE HUTS IN PROVENCE

Between the hill-village of Gordes and the 12th century Cistercian Abbey of Sénanque, north of Cavaillon, Vaucluse, the rocky and scrub-covered hillside is dotted, over a space of two or three square miles, with massive, thick-walled, false-vaulted stone huts of a primitive-looking pattern, limited to this locality : the locally so-called *bories*. There must be many scores of them, some standing perfect and still in use as tool-sheds or occasional shelters for peasants working in remote fields, others more or less ruined. In partially ruined examples, it is possible to examine and photograph the style of roofing (PLATE VI).

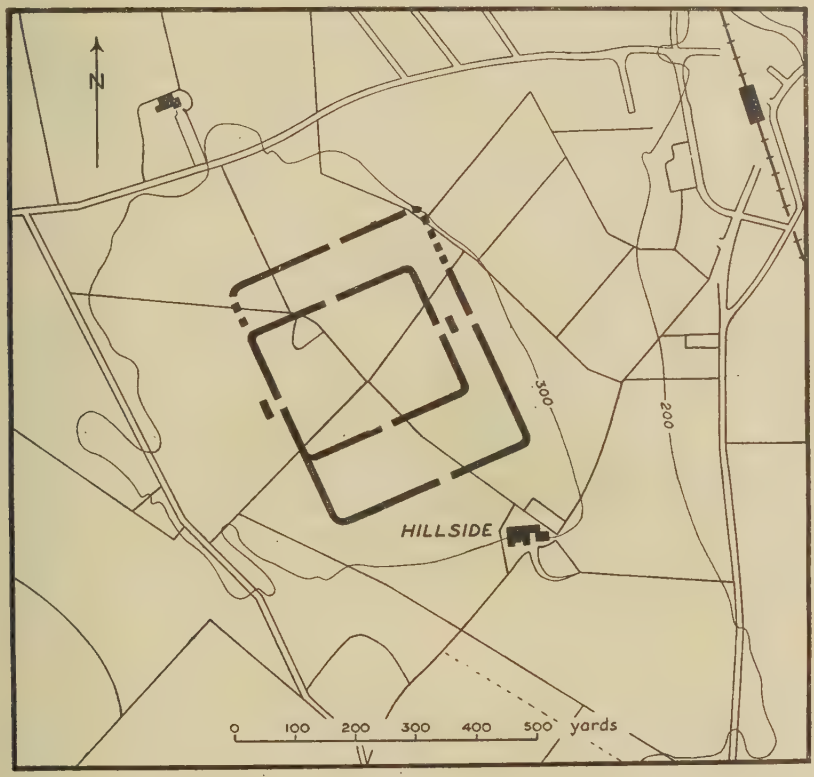
Bories usually measure externally some 16 or 20 feet in length, 10, or a little more, in breadth, and 8 or 10 feet in height ; a few exceed these dimensions by as much as 50 per cent. The walls are very thick in proportion—about 2 feet ; windows, framed by slabs set on edge, are usually very small (one may be seen PLATE VI, A) or, often, completely absent ; and except in rare and apparently hybrid examples, chimneys are absent. The door is always in an end, usually facing more or less south and downhill. Side walls are occasionally carried forward, sheltering the door, as in PLATE VI. The inward slant or curvature of the walls begins from ground level, giving a very solid and massive appearance. The chief difference from the beehive huts of the Bari region is that the *bories* are always rectangular in ground-plan ; they are vaulted, not domed. In many, an ingenious and interesting feature of their architecture is that an internal ' ceiling ' of large slabs is laid across as soon as the walls have come near enough together, while above this a ' relieving triangle ' is left, as above the lintels of the great portals at Mycenae. The walls are then continued up towards the roof-ridge, the gap being finally bridged by another slab when they are nearly touching.

Naturally these huts have sometimes been ascribed uncritically to a high antiquity ; but some are recorded to have been built as late as the 18th century, and French antiquaries attribute none of them to a date earlier than medieval. Their immediate descent is, it appears, from the huts of a leper-colony formerly maintained and tended here by the monks of Sénanque. The abject poverty of the lepers, plus the relatively high morale fostered by the monks, presumably constitute the social ' efficient cause ' by which this primitive but by no means despicable style of building was kept alive. As ' material causes ', we have the absence on this mountain-side (presumably even in the middle ages) of good timber for beams, and the presence, in unlimited quantities, of a limestone that splits easily and naturally into convenient slabs. The style of building is, like that of the Puglie huts, the mud-brick domed huts of North Syria, and, indeed, like other and more ambitious domes and vaults—the local response to the problem of roofing a space without the use of beams.

What leads one to think of the *borie* as a local survival rather than a medieval *ad hoc* invention, is the fact that except in one or two hybrid cases no use is made of the true or keystone arch. At the same time it is obvious that the local limestone lends itself much more naturally to the false vault. In any case, the huts of the Sénanque leper colony and those built in imitation in the immediate neighbourhood provide a well-attested example of a style of architecture, familiar in bronze-age structures from Britain to the Aegean, having been alive at least as late as the 18th century A.D. A. R. BURN.

ROMAN CAMPS AT DUNBLANE

In his recent book on Roman Scotland Mr Crawford has discussed the position at which the Roman road to Strathmore crossed the Forth [1]. The passage of this river with its broad belt of low lying mosses is a critical point on the road, the course of which is uncertain between the signal-station at West Plean and the fort at Ardoch. In spite of the absence of direct evidence, Mr Crawford is surely right in supposing that the crossing was near Stirling, a garrison-town in medieval and modern times.



The discovery by observation from the air in 1945 of two Roman marching-camps near Dunblane lends support to this opinion (PLATE VII and FIG. above). The site is on Hillside farm[2], on the south-west side of Dunblane, three miles north of the Forth and seven south-west of Ardoch. There are very few camps in the occupied portion of Roman Scotland that do not lie beside military roads, and this discovery confirms the view that the Roman line of advance to Ardoch was along Strathallan.

The camps, one of which is inside the other, lie on uneven ground which forms the top of a rounded hill 370 feet O.D. No traces are to be seen on the surface, but cropmarks of the ditches were visible from the air and appear clearly on photographs taken

[1] *Topography of Roman Scotland*, 1949, pp. 18–27.

[2] O.S. maps, 6-inch scale. Perthshire, CXXXII, NE. National grid ref.: 27/776006.

over a series of years[3]. It has been possible to plot from these photographs almost the whole outline of both camps. The smaller camp measures about 700 by 900 feet. Four gates have been noticed, at two of which there are traverses. The west side of this camp coincides with part of that of the large camp (approx. 1325 by 1075 feet), which appears also to have a single gate in each side. The gate on the west is in that part which is common to both camps.

J. K. St. Joseph.

SCARAB FROM SENNAR (PLATE VIII)

It may be of interest to add to F. Addison's interesting paper, 'Archaeological Discoveries on the Blue Nile', in ANTIQUITY, March, 1950, that I have now no doubt that his sites 3 and 5 at Sennar are the same cemetery. I spent a few hours there in May 1944, and was able to establish that there is in this area, just below the Sennar Dam, between the low river bank and the garden protection wall the remains of an occupation site which is being washed away by the scour from the sluices of the dam, and would have been all removed already but for being partially cemented with kankar. Site 3 was a grave dug into the eastern edge of this occupation site. In 1944 immediately on the western side of the occupation site, where the scour had cut down to bedrock there were visible the remains of several large oval graves cut into the sandstone bedrock and roofed with sandstone blocks and boulders. Most of the roofs had fallen in, and one grave entirely exposed had been recently robbed, and near it was a broken black ware bowl similar to those from the grave that is Addison's site 3.

In 1940 a fine scarab of King Shabako (*c.* 710–700 B.C.) was picked up on the river bank here. It probably came from the occupation site, finds from which include sherds of pottery with thick rims and of fine highly burnished painted ware, and stone rings, all typical of Jebel Moya, which was no doubt partly contemporary with King Shabako, while the graves are all later (Meroitic). This scarab is No. 3643 in the Khartoum Antiquities Collection and the other finds mentioned above Nos. 4762–4.

A. J. Arkell.

[3] Cambridge University Collection of Aerial Photographs, nos. A 27–8, D 23–4, taken by J. K. St. Joseph, July 1945, and Q 51–7, U 61–3, BD 68 taken in subsequent years. The site was visited on 25 August, 1945.

Important New Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

TRUTH BEFORE BEAUTY, or, the Incomplete Photographer, by ALISON FRANTZ. *Archaeology*, III, 4 (Winter, 1950), 202–14. [Much-needed guidance in photography for the instruction of archaeologists. We have often girded at them in this journal, but the standard seems to fall rather than to rise, through neglect or ignorance of the most elementary rules and principles. Reprints may be obtained from *Archaeology*, 100 Washington Square East, New York 3: 35 cents each, 10 for $3·00].

ROMANESQUE ARCHITECTURE IN ENGLAND, by SIR ALFRED CLAPHAM. Published for the British Council by Longmans, Green & Co., 1950. 2*s*. 48 pages. [A useful handbook by the late author of the standard books on this subject].

A FIELD SURVEY OF THE 'ROMAN RIG' DYKE IN S.W. YORKSHIRE, by F. L. PRESTON. *Trans. Hunter Arch. Soc.*, VI, 1950, 197–309. [An admirable piece of much-needed fieldwork undertaken in conformity with the methods drawn up by the Research Committee of the Soc. of Antiquaries, London, *Ant. Journ.*, XXVI, 1946, 175–9].

THIRTEENTH CENTURY DOMESTIC ARCHITECTURE IN ENGLAND, by MARGARET E. WOOD. *Arch. Journ.*, Vol. CV, Supplement, 150 pages. [This is a valuable piece of research, fully illustrated by plans and illustrations].

SOUTH AFRICAN PEASANT ARCHITECTURE: Southern Sotho Folk Building, Nguni Folk Building, by JAMES WALTON. *African Studies*, Vol. VII, 1948; VIII, 1949. [Discusses the character and spread of house-types; fully illustrated by photographs, drawings and plans].

SPÄTANTIKES SILBER AUS BRITANNIEN, by TOBIAS DOHRN. *Mitteilungen des Deutschen Arch. Inst.*, Bd. II, 1949, 67–139; 23 half-tone plates. Verlag F. Bruckmann, Munich, 1950. [This learned, but uncalled-for, new publication of the Mildenhall hoard is written by an art-historian who is ignorant of much of the relevant material. Even so it is surprising to find no mention of the authors whose Handbook has been used without acknowledgment].

PREHISTORIC TOWN PLANNING IN CRETE, by R. W. HUTCHINSON. *Town Planning Review*, Vol. XXI, no. 3, Oct. 1950, 199–220. [An excellent survey, fully illustrated by plans, several in two colours].

Reviews

VOUNOUS, 1937–38. *By* ELEANOR and JAMES STEWART. (Publications of the Swedish Institute in Rome, XIV). *Lund*, 1950. *4to pp.* 394. 107 *plates* (3 *in colour*) : 285 *illustrations*. *Price* 225 *Swedish crowns.*

This sumptuous volume records the excavation of an early Bronze Age cemetery near Bellpaise on the north coast of Cyprus, directed by a young Australian, James Stewart, and his wife, generously supported by the late Sir Charles Hyde, and printed by the Svenska Institutet in Rom and the Humanistiska Fonden. The text is in English, and I have found only one misprint ' Fracsinus ' on p. 372.

The general results of the excavation were announced in ANTIQUITY, XIV, 1940, p. 204–9, pl. II, and the principal date-mark, a Palestinian jug (pl. XCIV, a) was discussed in *Palestine Exc. Quarterly* 1939, 162–8, 1940, 152–9. Here the tombs and their contents are so fully described and illustrated that further exposition is unnecessary.

Vounous has now four excavated areas ; sites A and B dug by the Stewarts, another by Mr Dikaios for the Cyprus Museum (*Archaeologia*, LXXXVIII) and another by Dr C. F. Schaeffer for the Louvre (*Missions in Chypre*, 1936). Through the generosity of Sir Charles Hyde, part of the excavator's share has come to the Birmingham Museum of Art, the remainder to the Nicholson Museum at Sydney, of which Mr Stewart is now a curator.

The conditions for excavation were exceptionally favourable. The tombs, small caves with horizontal entrance, are cut in a gentle slope. The roofs have been denuded till they collapsed, but by that time rainwater had seeped in and deposited deep silt which protected the contents, though it disintegrated most of the bones. Only 16 skulls were recovered, ranging from *cephalic index* 92.6 to 78 05, with four above 88 and five between 85 and 80.7. Rainwater had also floated vases about and stranded them in great confusion without breaking very many.

The burials are mostly of Early Cypriot I, with red polished pottery without ornament ; the remainder of Early Cypriot II with richly incised patterns ; a few early vessels have the ' reserved-slip ' ware inherited from the Chalcolithic of Erimi and Choirokitia on the south side of the island ; and one or two anticipate the basket-painted ware of Early Cypriot III. The whole settlement thus slightly overlaps the cemetery at Lapathos further west, published by the reviewer in *British School Annual*, XLI.

Most of the tombs contained more than one interment, of various dates, and some of the contents were copious, and of high quality. Some of the bodies were extended (A87, B149, 131) others in contracted position (A90, 101, 104, 115) ; none certainly buried sitting like many at Lapathos. Of funerary ritual there was little trace, except one bronze dagger ' killed ' by bending (p. 100). Many of the door-slabs were in place, some reinforced with a pile of blocks ; tombs (114, 116, 117) had recessed façades, a quite new feature in Cyprus, though known in the Bronze Age of Sicily.

Copper flat-celts (here called ' axes ') leaf-shaped daggers (one a finely grooved one pl. CV), tanged daggers or spearheads, pins and one tweezer occurs, but metal was evidently rare. Analyses by Prof. C. H. Desch showed only one example of 11 per cent bronze. One pin had the curious wound ribbon of bronze, already known (CVII) ; one dagger retained traces of a wooden hilt, apparently oak. Novelties are clay models of

daggers and their sheaths (III, XCVII–VIII) and of a brush (XCVI). There were a few figurines, flat with incised drapery (XCVI) nut, not two-headed as at Lapathos.

On the later pottery the incised basket-ornament is exceptionally fine, and includes figures of dancers masked as ram or deer, and clad in bristly skins (XCIII) : these are quite new. The usual bowls (LXXXI–V) on high foot, with heads and figures on the rim present bowls, birds, deer, oxen, rams, a pig, a few flowers (new) and one human head (XX, 17). One large vase has a wine-press and tub (LXXXVIII). It is remarkable that there are no pig-bones among so many oxen and small cattle ; and that—though the oxen are recognized as *Bos longifrons* or *taurus*—the figurines have the usual spreading horns. Also that there are no such modelled groups or scenes as on Dikaios' site close by.

Occasionally later intruders had left Iron Age pottery (p. 156), once clearly as an appeasement (p. 316).

A very rare find is the impression of a coiled-ware basket, expertly discussed by Mrs Crowfoot, with Badarian, Ghasulian and Swiss Lake-dwelling parallels. This supports the current derivation of the incised ornament from basketry.

The photographs and tomb-plans show how carefully this excavation has been conducted ; it is indeed quite the finest discovery of this period. A special word of praise is due to the three coloured plates of ' red-polished ' and ' reserved slip ' vessels. Thanks also for a new word ' grog ' for the admixture of straw and silt in the pot-clay.

JOHN L. MYRES.

FOSSATUM AFRICAE : RECHERCHES AÉRIENNES SUR L'ORGANISATION DES CONFINS SAHARIENS À L'ÉPOQUE ROMAINE. *By* JEAN BARADEZ. *Gouvernement Général de l'Algerie. Paris* (Arts et Metiers Graphiques), 1949.

This book, already a classic, is the record of a post-war archaeological air-survey of a large segment—750 kilometres from east to west—of the hinterland of Algeria and the Tunisian border. Where feasible, observation and photography from the air have been followed, as they should be, by ground-survey and a little digging. But for the most part Colonel Baradez has ' fouillé des yeux ', employing the modern technique of continuous photographic cover from the relatively high altitude of 15,000–20,000 feet, in contrast to the low-level snapshots normal in the pre-war era. The method is well adapted to the initial survey of a far-flung and complex frontier-system, if sometimes rather too wholesale for the detailed examination of individual sites requiring selective lighting. With a few exceptions, Colonel Baradez's photographs are, however, of high quality from every point of view. Where they fall below standard, one of two causes may be blamed : the distance of many of the sites from base, or the inadequacy of the method of reproduction. The inky recesses of the collotype or equivalent process used for this work, as for Poidebard's famous report on the Syrian *limes*, blot out much which might have been rescued by good close-mesh half-tones from carefully printed negatives. Incidentally, the omission of titles to the illustrations is a constant nuisance to the reader and is inexcusable. The publication is sumptuous rather than technically perfect.

Apart from these minor though not negligible defects, the work exacts nothing but praise. It is indeed a landmark in Roman studies and, great though its immediate value be, its highest merit is that it systematically opens up a new field for historical and archaeological research. It does for Algeria—but with greater skill—what Poidebard did in 1934 for Syria. Only a few of its results can be noted here. The book itself must be within the reach of every Romanist.

The African salient extending from Carthage westwards towards Algiers and southwards to the Gulf of Gabes falls naturally, like most of the African littoral, into three

zones: the coastal region, the *gebel* or mountainous hinterland, and the desert *par excellence*. The *gebel*, writhen into valleys fertilized by seasonal moisture, presented a dual problem in that it was potentially productive and at the same time vulnerable to improvident nomads. It had to be simultaneously developed and protected, and Rome sought to combine the two aims in a single main objective. A considerable peasant population was anchored to irrigated field-systems and an appreciable part of the responsibility for local defence was at the same time devolved upon it. A similar policy has been demonstrated recently by Mr Richard Goodchild in Libya, and is partially implicit further east in Poidebard's evidence.

But the special interest of the Numidian frontier rests on its points of difference from that of Libya. With a patrolled road-system of which the framework was planned at least as early as Trajan (fortress of *Ad Majores* on the southern fringe of the *gebel* founded in A.D. 104) was associated at some uncertain period or periods a complex of linear works furrowing the desert 'comme le sillage d'un bateau sur la mer', a feature without parallel in Tripolitania or Cyrenaica. The contents of these cross-country ditches, which are 4–10 metres wide, were thrown up on the inner or sometimes on both sides. Elsewhere the ditch is backed by a dry-stone wall 1½–3 metres thick. Occasionally, on rocky ground, the wall has no ditch. Stone watch-towers, 3 metres or more square, are placed before or behind or, now and then, across the line at points of vantage, their number varying with the terrain. Forts are related to the line, but not in the rigid pattern characteristic of the nuclei of the British and German frontiers; rather do they indicate an elastic frontier-zone some 60–80 kilometres wide, with strongpoints fore and aft. Within this zone, the actual fosse, or rather fosses, demarcated the steppe from the (former) sown, as indicated by wide expanses of derelict field-systems which have themselves long reverted to desert. In this capacity the fosse-system did not constitute an integral unit. Whilst long stretches of it, linking natural obstacles, did in fact continuously delimit the Sahara to a full extent not yet ascertained, other detached works of a similar kind occur in the hinterland as local *limites* between cultivated valleys and the adjacent hills. There they served as warning obstacles to infiltrating marauders and doubtless depended primarily for reinforcement upon the local *limitanei* or *goums* rather than legionaries or auxiliaries, who must have been pretty thin on the ground. These native levies, as on the Syrian *limes* west of the Gebel Sinjar (see Poidebard), appear sometimes to have fenced themselves with irregular fortifications of their own devising and of entirely un-Roman aspect. (The present reviewer may be permitted to interpolate that he long ago suggested a similar context for certain hill-forts of the Roman period in Wales).

Important sections of the book deal with climate and agriculture. In regard to the former, the author comes down on the side of human interference or neglect rather than of unaided nature as the dominant factor in the desiccation of these lands. In doing so, he conforms with conventional opinion. More surprising is the extent of his evidences for ancient agriculture in and about the *gebel*. He calculates that in the region of the survey the once-cultivated area is in the order of 2½ million acres—an astonishing figure if compared with the desolation of today. What a prodigious achievement in respect of irrigation alone is implied by it! Setting aside any extensive cultivation of date-palms as extravagant of water, Baradez gives priority to olives, grown often in terraces and represented still by actual roots and by innumerable olive-presses; with a second priority to such cereals as periodical overflow might sustain. Small square or oblong fields, distributed in clusters or oases, were the rule.

In the light of all this production, it is perhaps scarcely enough to describe the Numidian frontier as a 'zone-tampon entre l'Empire et les Barbares'. It was in fact a

good deal more than a mere buffer of land-tied peasantry between the nomads and the coast. It *was* that, but it must also have been something a great deal more positive; it must have produced a very serviceable share of the surplus of olives and corn for which Africa was famous. It would be of much interest to economic historians to ascertain the chronology of this development. When were the various elements of the frontier-system laid down?—in particular, the linear works which mark a definitive moment in their evolution. Only excavation can provide the answer. The Theodosian Code of A.D. 409, addressing the Vicar of Africa, refers to the 'lands conceded to the *gentiles* by the provident humanity of the ancients (*antiquorum*) for the purpose of ensuring the maintenance of defence of the *limes* and the *fossatum*'; but whether the 'ancients' were of the 2nd, the 3rd or (much less likely) the 4th centuries we do not yet know. At *Gemellae*, a key-point close behind the Saharan line, a fort, then or later enclosed within an outer fortification of urban type, has produced an inscription of A.D. 126–7, and imagination warms to Hadrian as the appropriate architect of the *fossatum*; but later possibilities, fairly stated by Baradez, must be given weight until digging provides the answer. Meanwhile we may look with hope and even with excitement to the continuation of these brilliant air-surveys into the adjacent region of Mauretania on the one hand, and on the other hand to a junction with Goodchild's ground-surveys of Tripolitania. In one way and another, the map of Roman Africa is taking shape. R. E. M. WHEELER.

THE PROPER STUDY OF MANKIND. An Inquiry into the Science of Human Relations. *By* STUART CHASE. *Phoenix House, Ltd. pp.* 288, *Oct.*, 1950. 16*s.*

Mr Stuart Chase is an American whose special subject is economics, and it is in accordance with existing tendencies in sociology and social anthropology that these several disciplines should be treated as a single field in which they overlap. His book has clearly been written to give a much-needed boost to the social sciences, in the application of which to practical affairs he sees a possibility—perhaps the only possibility—of the salvation of mankind. He explains at length what culture means and the overriding importance of their culture to the individuals in any social group. He indicates the importance to social anthropologists of psychology, semantics, and economics, though he concludes, regretfully, that economics is not a science—at any rate not yet. He discusses methods of learning, the importance of communications in the widest sense, and the reformation of reformers. He poses a number of problems that require solution and suggests how it may be possible to get to work on them profitably. Above all he insists on the necessity for a scientific approach. In order to ensure this he devotes a chapter to the illustration of the characteristics, as he sees them, of the scientific method. As it is clear that many social anthropologists and sociologists have not employed the technique of the laboratory, he subdivides the social disciplines to cover those who do and those who do not follow what he regards as strictly scientific methods. He is severely critical of any sort of woolly thinking and of what he calls 'abstraction trouble' from which social science, he says (and quite correctly), has too often suffered. But his own terms, the terms with which he like any other sociologist must work, are themselves not always capable of sharp definition, and one may continue to doubt in spite of him whether either social anthropology or sociology is susceptible of the same approaches as the exact sciences. The trouble is that the student of the material to be studied is part of the material himself, and being human is changing too fast for his observations to have permanent validity. But whether Mr Chase is right or wrong in regarding social anthropology as capable of being developed into an exact science, he has rendered it a very valuable service as well as a tribute for which acknowledgment is due.

Consciousness of indebtedness, however, need not blind us to certain imperfections. It is probable that the fact that the author is primarily an economist explains a certain naïvety which is apparent from time to time when he writes of anthropology and of anthropologists. He can hardly have read Aristotle's *Politics* and yet classify the father of inductive reasoning, as he does, with Marx as a speculator who frames hypotheses and leaves the proof to others. Again he remarks that the Anglo-Saxon culture has been in existence since Chaucer, which is surely an arbitrarily late date, and that cultural anthropology ' was born with Morgan's work on the family systems of Seneca Indians ', had Sumner's *Folkways* (1906) as a landmark, and has been chiefly an American science from the beginning. We need not cavil much if Morgan is claimed as the greatest pioneer, but E. B. Tylor, who escapes mention, played no small part in the development of social anthropology. Again, Ogburn was far from being the first to measure the cranial capacity of Cro-Magnon man ; and religion does not determine rank in the Indian caste system. Now and then he even verges on the inept :—' Perhaps the real reason for the domination of Europeans over the so-called backward races is reflected in the . . . couplet

" Whatever happens, we have got
The Maxim gun and they have not," '

which may be true, but it was the Europeans who had invented and developed the Maxim gun ; they had not become possessed of it by chance. So too it is the merest caricature to attribute the conception of the primeval father, the old man who drove off the younger males from his horde of jealously guarded women, to a desire to flatter the Victorian paterfamilias, who would have found the comparison surprisingly odious.

Mr Chase's English style is occasionally open to criticism. That ' due ' may be used as an adverb must we fear be conceded to an American author, but he is certainly not justified in referring to himself in six consecutive lines as ' I ', as ' we ', and as ' the author '. His index is limited to the names of authors quoted, between three and four hundred of them, and it was left to his publishers to compile a bibliography for those wishing to pursue his subject further. J. H. Hutton.

STUDIES IN LATE QUATERNARY DEPOSITS AND FLORA-HISTORY OF IRELAND. *By* K. Jessen. (*Proc. Roy. Irish. Acad., Vol.* 52, *B., no.* 6). *Dublin. Hodges Figgis and Co.* 1949. 205 *pp.* 8 *maps*, 16 *plates.* 25*s.*

In the early days of quaternary geology, the geologist looked to archaeology to supply the zone fossils on which correlations might be based. As the techniques of the study have improved and as more data have become available, the debt owed to the archaeologist is steadily being repaid, particularly in the field of palaeobotany. Professor Jessen's results, based on two summers' field work in Ireland provides the ecological background against which archaeologists must set the developing human cultures. In all, forty-nine lake or mire deposits are described, a number of which have yielded objects of archaeological significance (e.g. the Larnian and Neolithic cultures of Cushendun ; the Early Bronze Age lance-head from Derrytagh North).

Although the greatest value of Jessen's work lies in the realm of botany, important archaeological facts and conclusions emerge. Using a revised zonation of Irish pollen diagrams which equates Irish, English, and Danish zones, the author demonstrates the similarities and contrast between vegetational, topographical and cultural development in the post-glacial period of these countries. Thus, he shows that in Ireland the great post-glacial marine transgression continued into the Atlantic period and at its maximum

was contemporary with the Late Larnian cultures of Island Magee and Cushendun, and pre-dated the Neolithic occupation of the latter site. This accords well with England where Godwin has shown that the post-glacial eustatic rise in sea level culminated in the early Atlantic, and with Denmark where the ' late Atlantic ' transgression took place just before the Neolithic period.

The transition between the pollen analytical zones VII b and VIII is frequently marked in Irish bogs by a stratigraphical horizon (RS.C) indicative of a renewal of active bog growth, which seems to correspond with the Grenz-horizont, or RY III, in Great Britain and Europe. Archaeologically, RS.C, seems to fall between finds of Middle Bronze Age (e.g. spear heads at Dernaskeagh and Cloonacool ; axes at Derryfadda Lower) and those of Late Bronze Age (e.g. sword from Canbo ; axe from Castlelackan), whilst the wooden shield of Cloonlara appears to be directly associated with the recurrence surface itself.

The reaction of the human population upon the vegetation can be traced, to some extent, from the pollen diagrams. The effect of Neolithic farming on deforestation is not so clearly demonstrated in Ireland as in Faegri's diagrams for S.W. Norway, and Godwin's work on the East Anglian Breckland, but as in both these localities Neolithic activity is associated with a period shortly after the final decrease in the frequency of elm pollen, which marks the transition from zone VII a, to VII b. The main agent in the deforestation of Ireland appears to have been the post-glacial climatic deterioration which characterised the sub-Atlantic period, but this effect was probably enhanced by progressive forest clearance as a result of human activity.

Professor Jessen's book is a notable contribution to historical plant geography, to climatology and to archaeology alike. D. WALKER.

GUIDE TO THE GREEK, ROMAN, ENGLISH AND CHINESE COINS, Ashmolean Museum, Heberden Coin Room; *pp.* 51, 9 *plates*, *Oxford.* Printed for the Visitors and sold at the Ashmolean Museum. 1948. *2s 6d.*

A few years ago a student coming to England to study coins would hardly have found satisfaction in any public collection but the Coin Room of the British Museum. Today, the Museum has several rivals, of which the Heberden Coin Room may be deemed the chief. The appearance of this little guide to its treasures is a timely reminder of its activities.

The Ashmolean Collection has been built up out of many different pieces. Several famous historic collections, a succession of minor gifts, collections of colleges and, finally, some munificent gifts and judicious purchases of recent times of the historical collections. The most interesting are those of Archbishop Wik (1657–1737) and Canon Philip Barton (died 1774). The ' praemia honorari ' of Elias Ashmole, founder of the Museum, have their peculiar interest and are illustrated on Plate I. The mint of Oxford is well represented ; and there is a very full collection of tradesmen's tokens of Oxford, City and County, in the 17th century.

Of the acquisitions that in the past few years have so vastly enriched the Coin Room, the Evans Bequest must take first rank. Sir Arthur had collected, with unfailing taste and discrimination, in many widely diverse fields. Greek coins of South Italy and Sicily, of West Greece and of Crete, Roman coins of the Civil Wars, A.D. 68–69, of Carausius, and of the last years of the occupation. The Heberden Coin Room is now the richer for all this love and skill. One might mention for its unique interest the denarius of the revolt of Civilis, celebrating the surrender of Legio XV Primigenia. Sir Charles Oman was another life-long devotee of the Greek series. The Ashmolean has

recently acquired some splendid series out of his collection, notably of Tarentum, of Corinth and of the Seleucid Kings of Syria. A magnificent collection of English gold, given by Dr A. T. Carter, has immensely strengthened the English series, while Captain Laird has presented a collection of Chinese coins, that illustrates on broad lines the secular history of the Far East.

The book is well printed and the plates are excellent. Anyone who does not yet know that the Heberden Coin Room is well worth a visit may now accept conviction that it is. HAROLD MATTINGLY.

EXCAVATIONS AT THE NJORO RIVER CAVE. *By* M. D. and L. S. B. LEAKEY. *pp.* VI *and* 78, 14 *plates, text figures and map. Clarendon Press, Oxford,* 1950. *Price,* 12*s* 6*d.*

The Museums' Trustees of Kenya and the Royal Anthropological Institute are to be congratulated on facilitating the publication of this exemplary report on an important excavation carried out in Kenya in 1938. The cave is one of a series along the banks of the Njoro river which all stand at about the same height above water level, and the authors conclude that they must have been flooded during the Nakuran wet phase, provisionally dated to 850 B.C., and that therefore the use of the cave as a crematorium must be subsequent to that date. Some 80 cremated individuals were found. Associated with the remains were obsidian implements regarded as derivative of the Elmenteitan culture, potsherds, stone bowls, pestles and lower grindstones, also beads made of chalcedony, agate, microcline felspar, etc.; together with the charred remains of a wooden vessel, basket ware, gourds, etc. There was no trace of metal; but many of the bones and grave goods were heavily stained with red ochre.

The archaeological remains are described in six chapters by Mrs Leakey, consisting of introduction, obsidian industry, pottery, stone bowls, etc., ornaments and carbonized objects. The second part consists of a detailed report on the crania by Dr Leakey; and there is a final chapter written jointly by the two authors. The illustrations and diagrams are of the high quality which their previous work leads one to expect.

When the reviewer first saw the finds at Nairobi, he, like the late Mr H. C. Beck, who reports on a sample of the stone beads, was struck by their similarity to some Predynastic beads. The human remains are non-negroid; and there are other points that suggest a cultural connection with the Nile valley in Dynastic times. The C Group people of Nubia also wore leather, used bone awls, made stone pendants, stone beads and beads of bird bone similar to those from Njoro, and sometimes buried with their dead thick shallow stone bowls carefully rounded on the outside, which Steindorff (*Aniba* I, pp. 109 ff.) believed to be the equivalent of the Predynastic palette for the preparation of red ochre or malachite. In the same publication there is a comparative study of the C Group skulls from Aniba and the Batussi of East Africa made at the suggestion of Professor Weidenreich, which concludes that, while most of the C Group were more negroid, two extreme skulls were comparable to Batussi.

It is however a well-known fact that practices and habits are retained for thousands of years in conservative Africa, and the dozen pottery bowls represented at Njoro, while being not impossibly descended from the incised bowls of the C Group, are with their lug handles (one pierced) surely later. And the Leakeys point out that there is a tribe living on Mt. Elgon, a natural mountain refuge for backward people lying in the direction of the Nile valley, who till recently made sedge-seed beads similar to those used at Njoro to space out the stone beads. Their conclusion that the Njoro River cave is comparatively late, despite the absence of metal, is acceptable; but they seem to have made the same

mistake as Vignard at Kom Ombo, who found an ochre-stained lower grindstone and concluded that it was evidence that the Sebilians ground corn. Stone bowls, ' pestle-rubbing stones ' and lower grindstones were all found stained with ochre at Njoro, and yet it is concluded not that they ' were used for grinding ochre but that they were used for grinding grain of some sort, although none was found in spite of careful search and although conditions were such that it could have been preserved '. It seems that the charring on the inside of the stone bowls may well have occurred during the cremation of the corpse. The ' pestle-rubbing stones ' appear where most worn to have developed the keels and polished faces found on the ochre-grinders of Early Khartoum.

The fact that no stone borers were found among the tools at Njoro together with the fact that ' the beads made from semi-precious stones were invariably interspaced . . . with small sedge-seeds ' suggests that the conclusion that the stone beads were made by the Njoro people may possibly be wrong ; but against this it was not a living site and all the raw materials are found locally. Mrs Leakey on p. 26 says that it is not known for certain how stone beads were drilled and ground in the Stone Age, but small stone borers were certainly used for boring disk beads of microcline felspar in the Khartoum Neolithic, and chert drills with emery or crushed quartz as an abrasive were used for boring carnelian and agate beads in the Indus civilization (Ernest Mackay, ' Bead Making in Ancient Sind ', *Journal of American Oriental Society*, vol. 57, p. 6). The two roughly chipped beads of obsidian with natural perforation from Njoro were no doubt local products. Mrs Leakey herself provides evidence against the other stone beads being such in drawing attention to the extreme rareness of stone beads in East Africa. (The hexagonal carnelian beads with narrow parallel-sided perforations that she mentions are no doubt comparatively modern imports from Cambay).

It seems that the extraction of the central incisors of the lower jaw correctly stated on p. 76 to be widespread in modern Africa, should be distinguished from the extraction of the central incisors of the upper jaw, which had a wide range in mesolithic times, occurring in north and central Africa and in Palestine (*Early Khartoum*, p. 114 and Arthur Keith, *New Discoveries Relating to the Antiquity of Man*, pp. 211 ff.). The central incisors of the lower jaw were not extracted in mesolithic Palestine as stated on p. 76.

These comments are not however made in a critical vein, but in an attempt to share in the elucidation of the prehistory of Africa, to which this volume is a valuable and important contribution.

A. J. Arkell.

MAYA HIEROGLYPHIC WRITING : Introduction. *By* J. Eric S. Thompson. *Publication* 589, *Carnegie Institution of Washington*, 1950. *pp.* xvi+347, 64 *figs.* *In paper cover*, $7.00 ; *in Cloth binding*, $7.50.

Eric Thompson is uniquely qualified to write a book like this. He has studied the Maya, their archaeological remains in the broadest sense, their Colonial literature, and themselves, for nigh on a quarter of a century, and a long series of smaller publications bears witness to his knowledge of their glyphic writing. Only a slight acquaintance with Maya glyphs is needed to know that the subject is complex and difficult, but readers of this book will have some idea of the extent of its complexity and its difficulty forcibly brought home to them. As Thompson himself says, ' there can be no " Maya glyphs without tears " '. The title of ' Introduction ' may be deceptive. The volume stands by itself and is as full a treatment of the subject as can be expected for many years, but I believe that a glyphic dictionary will follow it.

Each chapter contains important material, much of it either new or available previously to a small circle of readers only. Most of this is for the most specialised of specialists, but through the whole book runs a golden thread, which the author picks up again and again. Mathematics and astronomy have their necessary part, but an approach exclusively from these angles cannot lead to an understanding of the Maya or of their writing. We must rely more heavily on their beliefs, their religious symbolism, their mythology and, to a lesser extent, on their everyday activities. Again he says ' Mysticism, religion and poetry completely dominate the hieroglyphic writing '. The basis of all this is the burden of time, in which the divisions of time are carried continuously through eternity by divine bearers, the gods of the numbers, and perhaps its highest expression is to be seen in the full-figure inscriptions such as that on Stela D. at Copan, depicting the end of a period, where the gods are seen carrying their burdens on head bands like porters, some of them ready to continue into the next period and some to yield their load to the next relay.

This is not the place to go into details but a few matters of some general interest may be noticed. The chapter on the principles of Maya glyphic writing includes a discussion of its nature, from which the fact emerges that much of it was what we call rebus writing, though some glyphs were ideographic, but there appears to be some difficulty, as we might expect, about fitting New World conceptions into an Old World classification. In the next chapter, the importance of the old 260-day calendar to both ancient and modern Maya for ritual and divination is emphasised afresh. Not the least important feature of the chapter on the approximate solar year of 365 days is a discussion of the designation of the first day of the month. It has generally been believed that the 20 days of the month were numbered 0 to 19, but Thompson gives reason to believe that they were numbered 1 to 19 with an additional intermediate day which could belong either to this or to the previous month, though it generally belonged to the former. In this case, the glyph which has been held to mean zero in this context, the ' spectacle glyph ', has some other meaning such as ' the seating of the month '. The next chapter, on methods of recording numbers, also includes a discussion of the zero question, this time associated with various glyphs which are not interchangeable with the spectacle glyph and must have a different meaning. He concludes that these glyphs imply completion and not zero, though they can be used in much the same way as zero in positional numeration. This is not to minimise in any way the Maya achievement in ' inventing zero ', but only to call attention to the fact that the conception differs from that of the Old World mathematicians; it may serve incidentally to correct some diffusionist assumptions. The chapter on the Long Count includes among much other matter a discussion of its date of inauguration and of why it arose. It is not yet possible to answer the first question, although it is unlikely to be earlier than Baktun 7. It was developed in response to the supremely important concept of the eternity of time, of which it may be said that a corollary was the desire to combine the various time counts into a harmonious system. Another consequence of the same thing is shown by the instances, given elsewhere, of Maya calculations of the day and month positions of period endings long in the past, running into hundreds of millions of years ago, and it is suggested that the Maya were eventually led to believe that time had no beginning. Other matters dealt with include the 819-day cycle, recently discovered by Thompson himself, the Venus cycle and the Moon glyphs, aids to computation and decipherment, and finally ' Glances backward and a look ahead ' in which the author picks out what he believes to be his own most significant contributions and suggests the lines which future research on the very large number of undeciphered glyphs might take.

The earlier chapter entitled, Period Endings, Anniversaries and Katun Counts, which deals with the inscriptions carved on stela or altar to celebrate resting-places in the eternal march of time, is particularly notable for the nobility of its concluding passage (pp. 206, 207), too long to quote in full but including the following words on the ending of a katun ' Each was celebrated in the antiphonal chants of the Maya. Each was honored by the painstaking hewing and arduous transportation and erection of great shafts of stone. Glyphs, laboriously carved, told in measured lines of the greatness of the event. The grand cadences of the Initial Series sang its glory and the concluding hieroglyphs echoed its praise; towering pyramids rose to exalt it and stone lintels intoned its majesty. Captives lost their lives in sacrifice to it; priests shed their own blood in its honor '. This is a token of the spirit in which this laborious and important work has been carried out.

G. H. S. BUSHNELL.

MEROWINGERZEIT. *By* GUSTAV BEHRENS (*Römisch-Germanisches Zentral museum zu Mainz, Katalog* 13; *Original Altertümer des Zentralmuseums zu Mainz*). *Mainz* 1947, *pp.* 83, *figs.* 165, *pl.* 8. *No price indicated.*

This publication is a Catalogue of Merovingian-period Antiquities in the Mainz Central Museum. Practically every piece listed is illustrated, generally by a line drawing, sometimes by a half-tone block. Except in certain instances where a fuller account is demanded, descriptions are very brief, but in conjunction with the corresponding sketch, give a reasonably clear and accurate picture of the object. Pages 1–35 cover antiquities from the cemetery at Schwarzrheindorf, comprising eighty-seven grave-groups and a quantity of loose finds. Some of the best pieces from grave-groups were sold to museums in Berlin, Bonn, Frankfurt-am-Main and Hamburg before the bulk of the material reached Mainz. These pieces, though not in the Mainz Museum, are also listed here, so that the grave-groups are given complete, in accordance with the excavator's record. The cemetery is rich in combs, beads, brooches and pins, but particularly in glass and pottery. The pottery includes an unusual proportion of wide bowls, and much of it is of hard, wheel-turned light-coloured often reddish ware, having a distinctly Roman look. After the grave-groups, unassociated finds from the same cemetery are dealt with by types. They include buckets, weapons, pottery and some interesting brooches, three in the form of equal-armed crosses. The description of the grave-goods is preceded by some general comments on the cemetery and bibliographical references to it. After the description of the grave-goods there is a list giving particulars of the pieces from the cemetery that went to other museums, with their present whereabouts, an analysis of the furnishing of men's and women's graves, and some brief archaeological comments on the finds.

The next group of objects (pp. 35–9) from the Heerdt Collection acquired in 1932, have no provenance and are not in grave-groups. The pieces themselves suggest a Middle Rhine origin. Several good bird-brooches, some pendant crosses, and others for attachment, and an exact parallel (previously published in *Mainzer Zeitschrift*, 1933, Taf. 18, 3) to the openwork disc-pendant with ' griffins ' from Andernach in the Ashmolean Museum (E. T. Leeds, *Early Anglo-Saxon Art and Archaeology*, Pl. XVII, c) may be mentioned.

The antiquities from the Fliedner Collection, purchased in 1928 (pp. 39–68), are more numerous. Weapons of all types, buckles, strap-ends, square-headed, radiate and round brooches, the latter mostly decorated with garnets or stamped-foil designs; circular openwork ornaments in geometric pattern, combs, ornamented pins and other miscellanea are well represented. The sheet-bronze ornamental mountings of a wooden

casket, showing stamped and repoussé ornament of concentric circles in rectangular fields bordered with broad bands of punched dot patterns, is particularly interesting (this piece is already published by Alföldi, *Acta Archaeologica* v, 1934, ' Eine spätrömische Helmform ' (S. 114. Taf. 9, 2). But the main interest of this collection lies in the wide range of ceramic types, including bottle-vases very similar to those from the Faversham cemetery, as well as handled and spouted jugs, stamped urns, and bowls ; about a hundred and twenty pottery vessels are illustrated.

The Catalogue concludes with a section devoted to miscellaneous Merovingian antiquities, most from Germany (pp. 69–81). Notable amongst these are a small pail ornamented with bronze mounts in the style of that from Beauvais (Leeds, *op. cit.* Fig. 4) and the Buire sur l'Ancre bucket (Leeds, *op. cit.*, Fig. 5) ; and an unusual handled and lidded bronze ewer (Abb. 158), paralleled at Dettingen (Veeck, *Die Alamannen in Württemberg*, S. 30, Taf. 20, B4). The quality of the eight concluding plates is naturally enough, in view of the prevailing shortage of materials in Germany, not of the best, but the publication as a whole is a useful picture-book and place of reference for Middle-Rhenish antiquities of the Merovingian period, which are so closely related in many ways with our own finds in Kent. R. L. S. Bruce-Mitford.

THE COFFIN OF SAINT CUTHBERT. Drawn by D. McIntyre ; introduction by E. Kitzinger. *Oxford University Press.* 1950. 15*s.*

This work consists of careful line drawings of the surviving parts of the 7th century wooden coffin, together with an isometric diagram illustrating its construction. The drawings, the work of the cathedral Architect, Mr D. McIntyre, shew the fragments as they have been mounted on new oak boards for their proper display. The short introduction by Professor Kitzinger summarizes the history and appearance of the coffin and indicates the iconographical scheme and the models on which it was based.

The drawings are the result of a new analysis of the fragments and the present reconstruction supersedes that put forward by Canon Greenwell over 50 years ago. The most important result is to shew that the overall length of the coffin was only 65½ inches—not about 6 feet 8 inches as was formerly thought. This and the other dimensions make it difficult to believe that the body retained the full size of a living person when first deposited in the coffin in 698, 11 years after the death of the saint. The new reconstruction also throws light on the interpretation of the figures, which Professor Kitzinger explains as ' a litany in pictures ' representing ' the powers whose protection is being invoked for the sacred relics '. The artistic models of these figures are of Mediterranean origin, from various sources. The present work is an excellent record of the material ; a fuller discussion of the artistic and literary problems is promised as part of a large volume now in preparation on the Relics of Saint Cuthbert. C.A.R.R.

BROWN MEN AND RED SAND : journeyings in wild Australia, *by* Charles P. Mountford. *Phoenix House, Ltd., London,* 1951. 18*s.*

Those who have taken a University course in anthropology have been taught to regard the Australian aborigines as interesting specimens of humanity ; but, excellent in some respects as the books about them were, they gave no hint that the aborigines were ' one of the most lovable races of men '. No one could read this delightful book without agreeing with Mr Cleland's opinion (just quoted, from his Foreword). Here at last is a real live anthropological book about one of the very few peoples who still subsist as nomads by hunting and collecting. It is a book that makes one think, for these nomads,

no freer presumably from original sin than we are, do not kill each other unnecessarily and are often on a higher moral plane than their civilised compatriots. Yet in respect of material culture they are pre-palaeolithic! Mr Mountford here describes a trek he made in the heart of Australia, the daily life, habits and religion of these charming people; and he illustrated his text with some of the finest and most dramatic photographs we have ever seen. That opposite p. 89 of a naked man doing a rock-painting is superb; it might be one of our palaeolithic ancestors decorating a rock-shelter 15,000 years ago. This is the sort of book that anthropologists might write if—but enough. It is one that all archaeologists and readers of ANTIQUITY should buy, for it will give them permanent pleasure.

The book is well produced and the few defects do not really detract appreciably from its excellence. The map is spoilt because the names on the stippled areas are illegible and there should have been a scale as well as that supplied by the coordinates. The course marked does not agree with the text on p. 160 (Wilson-Arana). The plates are unnumbered, so that there are no direct links with the text; but they are well integrated therewith, and such cross-references would have been easy and helpful. We should have welcomed a little more enlightenment on the extraordinary geology of the region; the book will be read by many to whom elementary geology is not quite a closed book. The apparent mixture of igneous and sedimentary formations is intriguing. But of course that is a secondary feature of the book, though it comes into prominence through the affection the aborigines feel for the rocks and stones of their native district. That is all we can find to criticize in a charming book. O.G.S.C.

SYBARIS, *by* J. C. CALLAWAY: the Johns Hopkins University Studies in Archaeology, No. 37. *Johns Hopkins Press, Baltimore* [*London: Geoffrey Cumberlege*], 1950. 131 *pages.* £2.

This book 'attempts, by drawing on all known sources, to present a somewhat fuller and more integrated account of the city as a whole than has heretofore been available'. Within these self-imposed limits it fulfils its task in an excellent and scholarly way; there can be little in classical literature relating to Sybaris that has escaped the compiler, and his point of view is refreshingly original. He has earned the gratitude of all future students, especially of excavators to come, by thus collecting between two covers a mass of scattered information. We can thus form quite a vivid mental picture of life in a famous city during a dark period of history (*c.* 720 or 710 to *c.* 510 B.C.). That is a considerable achievement; it might have been even greater had the 'all known sources' included the modern techniques of air-photography and field-work. The book would have been greatly improved by a few maps, including a reproduction of the largest scale modern map of the district. It is impossible without such to follow intelligibly the descriptions in the earlier pages of the book. These at least were within reach, even if field-work was not feasible and no air-photographs available. But surely here, if anywhere, the out-door technique is imperative; for the site of Sybaris remains to be discovered. It has been suggested that it may be beneath the sea, for in this unstable region earth-movements produce subsidence; the coast at Messina sank visibly at the time of the great earthquake of 1906. But it seems improbable that so great a subsidence as would be needed to obliterate all traces could have taken place. It is far more likely that no one with the necessary field-qualifications has ever taken the trouble to look for the site, either on foot or in an aeroplane.

Sybaris gave a new word to the ancients and to ourselves, and 'sybaritic' is used in a depreciating sense; a 'sybarite' is 'an effeminate voluptuary or sensualist' (O.E.D.).

But, as our author says, the term was not deserved, and may well have originated from envy of people who had raised their standard of living above that of their contemporaries. Moreover it must be remembered that we have no contemporary accounts of Sybaris. How little the later stories can be trusted is shown by the amusing story told by Timaeus of the Sybarite at Sparta who said he would rather die than endure the Spartan kind of life. Now Timaeus wrote more than a century and a half after Sybaris had ceased to exist, and archaeology has shown that the early Sparta was by no means ' Spartan ' in the usual sense. The story is completely anachronistic.

Sybaris was an enterprising Greek colony in what was still, in the 8th or 7th centuries, a new world just coming within the ambit of civilisation. Its prosperity was due to trade, for its inhabitants had cut out the maritime route from the Greek world to Central Italy by establishing overland ones across the toe of Italy. As Randall MacIver pointed out (quoted on p. 49) this achievement was comparable on a smaller scale with the replacement of the Cape route to India by that through the Suez Canal. The result was a great increase of wealth and population. Our author hints at a certain historical analogy between Magna Grecia and the New World of America, and it seems apt. One might venture a further comparison between the later Greek world impoverished by warfare which looked back enviously to an imagined golden age, and the distracted Europe of today which does the same. Nor was that golden age wholly imaginary. But neither was it necessarily reprehensible, and there is no evidence to show that there were more ' effeminate voluptuaries or sensualists ' in Sybaris than there were in Paris or New York before the wars began, unless those terms can be applied to all who enjoy the legitimate pleasures of life and have a high standard of hygiene. In any case neither the Athenians nor the Romans were the ones to cast stones. O.G.S.C.

ISRAEL EXPLORATION JOURNAL. Vol. 1, no. 1. *Jerusalem*, 1950–51. *Published quarterly. Annual subscription rate* I£2 ; *in United Kingdom*, £2, *to Luzac and Co. Ltd.*, 46 *Great Russell Street, W.C.*1.

All students of Near Eastern archaeology in general and of Palestinian archaeology in particular will welcome the appearance of this new journal, which makes available to those who cannot read Hebrew the results of archaeological research in Israel. Reports reaching this country have shown that there has been much activity there, since the end of the Mandate, under the leadership of the Jewish members of the former Department of Antiquities of the Mandate, and of the Department of Archaeology of the Hebrew University. Building and agricultural projects have revealed many new sites and threatened known ones, and the archaeologists have been hard put to it to record all the finds. A journal which serves as a vehicle to make this material accessible to a wider public is indeed an acceptable addition to periodical publications.

The contents of the first number are : A New Neolithic Industry : the Yarmukian of Palestine, by M. Stekelis ; Caesarea : A Study in the Decline of a Town, by A. Reifenberg ; Sand and Soil in the Coastal Plain of Israel : A Study in the Rate of Accumulation, by M. Rim ; A Jewish Sepulchral Inscription from Caesarea, by M. Schwabe ; The Development of the Roman Road System in Palestine, by M. Avi-Jonah.

All are interesting, but space does not permit of a detailed discussion. The most important is undoubtedly Stekelis' article on the Yarmukian, which adds another facet to the growing knowledge of the Neolithic of Palestine. But in connection with it, perhaps the Editors should consider the policy of the Journal. Some articles may well be popular or semi-popular, as indeed is the article on Caesarea. But when they provide what is presumably the definitive publication, as far as English-speaking readers are

concerned, of an important piece of research, they should surely provide all the evidence needed to support statements. As far as the descriptions of the finds goes, this the article does do, though one would have wished for a drawn section to illustrate the stratification. Incidentally, in the table on p. 4, a metre has apparently got lost between layers B and C. It is when the author comes to the interpretation, that the treatment is hardly satisfactory. Stekelis dogmatically arranges the Neolithic chronologically as Usbian (from Abu Usba') *c.* 7,500 B.C. ; Yarmukian, *c.* 7,000 B.C. ; Jerichoan, *c.* 5,500 B.C. He neither gives any reason for his (remarkably high) dates, nor for a succession rather than a co-existence, which would appear to be not at all impossible, and he ignores the evidence which at Jericho seems to suggest the possibility of an indigenous invention of pottery during the Neolithic, and of a development there of the Neolithic from the Mesolithic. It is to be hoped that future articles of this importance will give more detailed reasoning.

Everyone will so emphatically wish the Journal well, that some criticisms of lay-out and production are made in the hope that they will be helpful. The reproduction of the plates is excellent and the format is pleasant, but surely extravagant, with unnecessarily wide margins and wide-spaced lines. Could not some of this be sacrificed to give us more for our money ? Compared with English journals, the fare is rather scanty. In future numbers, one hopes that the table of contents on the back cover will change places with the information about the Journal on the front one. As regards illustrations, it is far more satisfactory to have all objects on one plate reproduced at the same scale ; one can thus avoid the repetition of the rather ugly scales of Stekelis' plates, even with the aid of which the eye is deceived as to relative sizes ; on pl. III for example, every object is at a different scale. The air photograph of Caesarea would be far better reproduced on a larger scale as two separate plates, and one has a nasty suspicion that the southern section has been touched up, in fact faked. The figures in the northern section are ugly and obscure the photograph. To the present reviewer, however, the worst fault is the publication of the figures illustrating Stekelis' flints as a loose folder. It is made up of a number of separate figures which could perfectly well have been included in the text, and, moreover, it will be a librarian's headache, as there is no margin by which it could be bound in to a complete volume. There is nothing to indicate the unit of the scales, presumably centimetres, and again it would be far better if the scales were uniform. The scale of fig. 13 has no figures on it.

These are faults, but ones which can easily be corrected. If they are, the Journal may well become first-class. Our best wishes go to it. K. M. KENYON.

ANTIOCH MOSAIC PAVEMENTS. *By* DORO LEVI. *Princeton University Press (London : Geoffrey Cumberlege).* 2 *vols.* 200*s net.*

The mosaic pavements form the most important group of monuments discovered by the Franco-American excavations at Antioch. No other site has yielded so widely representative a series, or one whose chronology has better chance of being determined by stratigraphic evidence. Preliminary publication of all the pavements was made with commendable promptness in *Antioch-on-the-Orontes* II and III, but it was impossible within the limits of an excavation report to enter deeply into their many iconographic problems, or trace their stylistic development. These tasks were reserved for Dr Levi in the present work which is in addition ' an attempt to bring mosaic art as a whole into the wider picture of the history of art in antiquity '.

The result is exhaustive and exhausting. It would be ungracious to criticize too severely the English of foreign scholars who do us the compliment of writing in our own language ; but one cannot help thinking that some sort of editorial supervision should

have prevented nonsenses such as ' the marine train scores a crescendo ' or ' are you winning with peace, O seer ' ? Nor can the difficulty of an alien tongue explain everything. ' The art of smelling the perfume of a flower held to the nostrils ' must surely sound as much *de trop* in Italian as in English.

It is this exuberant pleonasm, in argument as well as language, that makes Dr Levi so exasperating to read. He seems incapable of moving from A to B without first circumambulating all the other letters of the alphabet. Thus, to take one example, the hat worn by Summer in the mosaic of the Seasons from the Constantinian villa (p. 234 f.) is eventually equated (wrongly, in my opinion) with the head-dress of a lady on an Egyptian textile in Leningrad. But before reaching this questionable conclusion we have to traverse the whole range of ancient millinery : *pilos*, *κυνή*, *petasos*, *θολία*, *δαλία*, *kausia* and the Hellenistic high hat (' an element of the elegant garb of women at the promenade '). It is all very erudite and impressive, but is our journey really necessary ?

From the welter of information thrown up by these encyclopaedic methods it is difficult to discern exactly what emerges. Much perhaps of incidental interest, even of importance, but certainly not a coherent and clearly defined history of mosaic at Antioch or elsewhere ; and no new contribution to the history of ancient art in general. Yet mosaic, and therefore Antioch particularly, has such a contribution to make ; for mosaic is, in Kurt Kluge's convenient phrase, the ' epochale wahlstoffe ' of later antique and early medieval art. Dr Levi was certainly handicapped by the fact that the technical aspects of the mosaics were reserved for separate treatment ; style and material are more closely related in mosaic than in almost any other medium. But one feels he is even more handicapped by a largely negative attitude to his subject (' we will not commit the exaggeration of denying to all of them any aesthetic value whatsoever '). It is not surprising that the most satisfactory section is that dealing with iconography.

Some miscellaneous points in order of appearance. P. 29 : Beazley *Panmaler*, p. 25, no. 71, lists four red-figure vases representing the infant Herakles strangling the serpents. P. 71 : Ovid inserted *madidas* between *Venus* and *exprimit* (*Ars am.* III, 224). P. 75 : could not the scene represent Aphrodite showing Helen to Paris ? P. 122 : except for the *pulpitum* reliefs the theatre at Sabraltha is usually held to be pre-Severan. P. 166 : *navium celerissimus* won't do. P. 184 : it is false to argue that the original of the Satyr and Hermaphrodite group must have been statuary, not painting, because an Antioch mosaic represents the front and back views of what is clearly a statuary version of it. It proves no more than that statuary versions did exist, a fact which their survival proves even more conclusively. P. 296 : *quiescentium* is the right form. P. 318 : read *φάγ[εται* for *φάγ[ονται*.

The book is well produced, misprints are few, and the plates are a revelation of how good full-tone collotype can be.

I. E. HAYNES.